Vol. 68, No. 3 May 1961

Psychological Review

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PUBLISHED BIMONTHLY BY THE
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
Prince and Lemon Streets, Lancaster, Pa.
1333 Sixteenth St. N.W., Washington 6, D. C.

$10.00 volume $2.00 issue

Second-class postage paid at Lancaster, Pa.

Send all communications, including address changes, to 1333 Sixteenth St. N.W., Washington 6, D. C. Address changes must arrive by the 10th of the month to take effect the following month. Undelivered copies resulting from address changes will not be replaced; subscribers should notify the post office that they will guarantee second-class forwarding postage. Other claims for undelivered copies must be made within four months of publication.

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VOL. 68, NO. 3 MAY 1961

PSYCHOLOGICAL REVIEW

EARLY VIBRATORY EXPERIENCE AND THE QUESTION OF INNATE REINFORCEMENT VALUE OF VIBRATION AND OTHER STIMULI:

A LIMITATION ON THE DISCREPANCY (BURNT SOUP[1]) PRINCIPLE IN MOTIVATION[2]

J. McV. HUNT AND HERBERT C. QUAY
University of Illinois *Vanderbilt University*

This paper describes an exploratory attempt to test one aspect of Hebb's (1949) theory of affective arousal and motivation and suggests a limitation on this theory. According to Hebb's theory, disruption of cerebral processes is emotional disturbance. One source of disruption is a discrepancy between present receptor inputs and the residues of past inputs of corresponding nature. Hebb (1949, p. 149) claimed that while mild discrepancies in sequences and qualities of inputs from the accustomed sequences or qualities may be pleasurable, large discrepancies will produce the disruptive emotional disturbance. One kind of discrepancy is absence of accustomed stimulation. As illustrations of this, Hebb used fear of the dark in children, avoidance of solitude in young chimpanzees (Köhler, 1925), and the irritability that has been reported to follow loss of normal auditory stimulation (see Hebb, 1949, pp. 252–253). Furthermore, it is the avoidance or termination of such emotional disturbance which may serve as a reinforcer for many possible instrumental acts, e.g., the avoidance of fear and the attack of aggression.

Rats reared under ordinary circumstances will, when placed in a vibrating cage, learn to press a bar to stop the vibration. Hebb's discrepancy principle suggests that rats reared in a vibrating cage (*a*) will learn to press a bar to reinstate the accustomed vibration once it is removed and (*b*) will fail to learn to press a bar to stop vibration. In this exploratory experiment, the first expectation was disconfirmed, the second confirmed. The findings suggest a limitation on the discrepancy principle. Moreover, these findings in combination with those from other recent studies suggest that various receptor inputs may have innate reinforcement value which places a limitation on the discrepancy principle in motivation and should be investigated.

[1] After the old story of the bride who had failed repeatedly to cook her husband's favorite bean soup "as Mother made it." One day she accidentally burned it. Then, after she had dished the burned soup for him in utter shame and with trepidation, she heard the accolade: "Now you've got it, Darling, just like Mother made it."

[2] This paper was written while the senior author was working with support from USPHS Grant M (1041) C2.

METHOD

The strategy of this exploratory demonstration has been to compare the rates of bar pressing between animals reared on a vibrating table and animals reared on an ordinary table when, in adulthood, (*a*) they were placed in a still test-cage provided with a bar which would start the vibration when pressed, and (*b*) they were placed in a vibrating test-cage where a bar press would stop the vibration.

Subjects. The subjects were six litters of white rats (Sprague-Dawley). The three litters reared with vibration numbered 9 males and 11 females; the three reared under stable conditions numbered 15 males and 6 females.

Apparatus. These rats were reared one litter to the cage. Each cage consisted of two 12" × 12" × 1.5" galvanized iron pans, one as top and one as bottom for a hollow square of wire mesh. To prevent rattling, foam rubber pads were used when and where necessary.

For the condition of vibration, these cages were placed on a thick foam rubber mat on a table the top of which was attached to the frame with four rubber mounts. Attached to the bottom of this table top was a 1,000-cycle motor. Vibration was created by mounting a 2-inch (length) steel bar (1 inch square) on the shaft with the hole in the bar off-center by 0.5 inch. The vibrating table used four cages, one for each of the three litters reared with vibration, and one to serve for exchange and storage purposes when cages were cleaned and when the rats were being tested. Within the same room, a similar but standard table with another four cages was employed for the three litters of controls.

The test apparatus consisted of a duplicate of the vibration table, but on it was a test-cage supplied with a Skinner-type bar arranged so that a bar press could either make or break the circuit that supplied the motor. Both the make or the break also started a timer which could be set to break the current to the motor which had been made by the bar press, or to make the current broken by the bar press after any set interval. In this experiment, this timer was set for 30 seconds. Thus, if the subject was put into the test-cage when still, one bar press got 30 seconds of vibration, or, if the subject was put into the test-cage when it was vibrating, one bar press got 30 seconds of a stable world. A counter was attached to count the bar presses. This arrangement had in it one defect: in both starting and stopping, the test-cage made a somewhat convulsive motion which differed some from the customary vibration. We could not get the vibration to start and stop as smoothly as desired.

Procedure. The experiment started with getting six female rats which were approximately 10 days pregnant from the Sprague-Dawley colony. Three of these were individually caged on the vibrating table, three on the ordinary table. The six females gave birth during a period of 4 days (November 25–28, 1957). These females remained with their litters for 30 days. All animals had food (Purina dog chow, supplemented occasionally with greens) and water ad lib.

Although intense vibration can be noxious (Schaefer, Ulmer, & Link, 1959) or even lethal (Schaefer, Link, Farrar, Wiens, & Dinsmore, 1959), growing up in vibration of the intensity used in this experiment did not hamper rate of growth. When the pups were weighed at 4 weeks of age, the mean weights were as follows: for vibrated males, 76.7 grams; for control males, 77.2 grams; for vibrated females, 72.4 grams; and for control females, 72.0 grams. Weights ranged between 62 and 86 grams. Greater homogeneity is seldom found with all animals living under similar conditions.

Testing began when the pups were 56 days old. It ended some 3 months later. The pups were tested individually. Each test took 24 hours. In the first series of tests, the pup was put into a still test-cage so arranged that a bar press got 30 seconds of vibration. To equate the age at which the vibrated and the control pups were tested, one from each sample was tested on alternate days (except for two occasions when, by experimenter error, vibrated animals were tested on successive days). After testing, each animal was returned to his respective condition of life in the spare cage provided. This first series of tests continued till 17 of the vibrated animals and 13 of the controls had been tested, when the apparatus broke down. When it was fixed, because it was important to complete tests by the end of April when other work was scheduled and because it appeared from the results available that it would be exceedingly unlikely that the vibrated rats would, as expected, turn out to press the bar on the average more often than the controls to get vibration, we turned to the second series of tests without giving the first test to the remainder of the pups. In the second series of tests, each pup, vibrated and control, was put into the test-

cage while it was vibrating. A bar press stopped the vibration for 30 seconds.

No special arrangement was made in the design of this exploratory experiment for control of either litter or sex difference.

RESULTS

The expectation that the vibrated animals would press the bar more often than the controls to obtain vibration, is contradicted. The vibrated animals actually pressed the bar less often ($M = 9.8$) than the controls ($M = 16.8$), and the difference is significant ($t = 3.397$; $df = 28$, $p < .01$). If use of the t test is questioned because the number of bar presses cannot be assumed to be an interval scale for the avoidance of or the approach to vibration, the difference is also significant with the use of the Mann-Whitney U test ($U = 55$, $p < .02$).

It is also quite clear from Table 1, this time as expected, that the control animals did press the bar more often ($M = 47.5$) than did the vibrated animals ($M = 10.1$) to stop vibration. This confirmatory difference is highly significant by either t test ($t = 5.85$; $df = 19$, $p < .001$) as Cochran-Cox corrected for nonhomogeneity by halving the pooled df, or by Mann-Whitney U test ($U = 99$, $p < .02$).

If one compares the numbers of presses by each group in the two kinds of test, it can be seen that the vibrated animals average about the same number of presses to get vibration ($M = 9.8$) as they do to stop it ($M = 10.0$; $t = 0.6$, $p > .30$; and $U = 125$, $.05 < p < .10$), while, on the other hand, the controls average only about a third as many presses to get vibration ($M = 16.8$) as to stop it ($M = 47.5$), a difference which is highly significant by the t test ($t = 4.49$; $df = 16$ as Cochran-Cox corrected for nonhomogeneity, $p < .001$) but not by Mann-Whitney U test ($U = 115$, $.05 < p < .10$).

TABLE 1

ARRAYS OF NUMBERS OF BAR PRESSES FROM VIBRATED AND CONTROL ANIMALS (SEX INDICATED) FOR FIRST SERIES OF TESTS WITH THE TEST-CAGE STILL AND FOR SECOND SERIES OF TESTS WITH THE TEST-CAGE VIBRATING

First Series Bar Press Gets 30 Seconds' Vibration		Second Series Bar Press Stops Vibration 30 Seconds	
Vibrated ($N = 17$)	Control ($N = 13$)	Vibrated ($N = 20$)	Control ($N = 21$)
	F 54		M 315
F 35			M 191
	F 29		F 92
M 24			M 69
	M 23		M 56
	M 23		F 48
M 20	M 20	F 44	
M 18			M 43
	F 13		F 41
	M 12	M 38	M 38
F 10	M 10	F 31	
	M 9		M 27
	F 9	M 22	
M 7			M 20
M 7			M 13
M 7		F 12	F 12
F 6	M 6		F 11
F 6	M 6	M 9	
M 4	M 4	F 8	
M 4		F 6	M 6
F 4		F 5	
F 4		M 5	
F' 4		M 4	M 4
F' 4		M 4	
M 3		F 3	F 3
			M 3
		M 2	M 2
		F 2	M 2
		F 1	M 1
		F 1	
		F 1	
		M 1	
		M 1	

DISCUSSION

If the convulsive motions at the beginning and the end of a period of test-cage vibration can be assumed to be of negligible effect, as we believe they can from observing the animals, apparently living with vibration from before birth can *diminish the negative*

reinforcement value of vibration, inasmuch as the vibrated animals averaged fewer presses of the bar to stop vibration than did the controls, but it *fails to give vibration positive reinforcement value* because the vibrated animals failed to average more presses of the bar than the controls to get vibration. Although the fact that vibrated pups and control pups had essentially equal weights at 4 weeks of age argues that vibration can hardly be considered to be noxious, this finding suggests that it must be *innately negative in reinforcement value*. In fact, the sort of operations employed in this exploratory experiment, even though the convulsive starting and stopping motions leave something wanting in the particular testing device used, provides an operational definition of *innately negative in reinforcement value*.

This notion that certain types of receptor inputs have innately negative reinforcement value, although only suggested by these results, is clearly supported by the results of at least two other experiments. First, in a test of the effects of being raised with continuous experience of a harmless bitter substance, Warren and Pfaffmann (1958) used sucrose octaacetate (SOA), a synthetic compound which has been found to be nontoxic when fed to rats and rabbits, which is intensely bitter to man, and which is avoided by guinea pigs and rats in a two-bottle preference situation. Guinea pigs were prematurely weaned at 2 or 3 days of age and raised in incubators. The experimental group drank only solutions of SOA. The controls drank ordinary water. When tested at 21 days of age in a two-bottle preference situation, the mean percentage intake of the SOA solution was 38.4 for the SOA reared and 13.1 for the controls ($p < .01$). Thus, like the rats raised on vibration, these guinea pigs raised on bitter SOA were less rejecting of SOA than guinea pigs raised on ordinary water, but they did not reject ordinary water. In fact, in spite of being reared on solutions of SOA, they tended to prefer ordinary water.

Second, in a test of the effects of being reared, following weaning at 25 days of age, under conditions of illumination which flashed on and off 60 times a second, Meier, Foshee, Wittrig, Peeler, and Huff (1960) compared the preferences, at 90, 97, and 104 days of age, of mice (strain CF/1) so reared with the preferences of control mice, reared under ordinary constant illumination, for a pair of communicating compartments one of which was illuminated by a flashing light and the other with constant light. Although those mice reared on flashing light avoided the compartment illuminated with the flashing light less than did the control mice reared under conditions of constant illumination, both groups showed preference for the compartment illuminated with constant light. Like vibration and bitterness, flashing illumination appears to have innately negative reinforcement value.

These three experiments suggest further that becoming accustomed to any type of stimulation which is innately negative in reinforcement value tends to reduce the intensity of an organism's tendency to escape from or to avoid that stimulation, but the absence of such accustomed stimulation does not evoke the emotional disturbance predicted by Hebb (1949) from his formulation of the discrepancy principle of motivation. Neither does the reinstatement of an accustomed form of such stimulation, with innately negative reinforcement value, provide positive reinforcement.

These findings do not deny the discrepancy principle; they merely set a limit upon it beyond that already

clearly recognized in stimuli which are painful or organismically damaging. Innately negative reinforcement value may thus be seen to be independent of either pain or noxiousness.

Neither do these experiments deny that types of stimuli which are innately negative in their reinforcement value can be made positive by experience. If such stimuli as vibration, bitter SOA, and flashing lights were incorporated into sequential organizations with consumatory acts, they could undoubtedly be made positive just as faradic shocks on the flank of a dog acquired the capacity to elicit salivation, presumably the counterpart of an approach response, when Pavlov (1927) utilized faradic shocks to signal the coming of food for hungry dogs. But in such a case, as Hebb (1949, p. 189) points out, the stimuli with innately negative reinforcement value would be integrated into a phase sequence, and one of positive outcome.

The Concept of Innate Reinforcement Value

This notion of receptor inputs having innate reinforcement value deserves discussion. In one sense, the notion may be seen as revisiting Wundt's early (1874–1887) doctrine that feeling is an attribute of sensation, a doctrine which he later (1896) gave up for his tridimensional theory of feeling (see Titchener, 1908). But in other senses, this notion is quite different from Wundt's. Psychology is no longer concerned with describing "immediate" experience, and only insofar as felt and reported pleasantness of stimulation happens empirically to be associated with positive reinforcement value, do hedonic value and reinforcement value become equivalent. The degree of their equivalence is an empirical question worthy of more investigation than it has received.

Our hypothesis that stimuli may have innately positive or negative reinforcement values may also be seen as revisiting Troland's (1928) conceptions of beneception and nociception, i.e., sensory processes indicating conditions, respectively, favorable and detrimental to survival. Even though survival in evolution may conceivably account for positive and negative reinforcement value, however, Troland's postulation of such a relation differs substantially from our empirically more-readily-determined reinforcement value.

Except for stimulation which is inherently painful or of such strong intensity as to be noxious (Miller & Dollard, 1941), most psychologists of the past 40 years have been reluctant to consider reinforcement or hedonic values as innate accompaniments of receptor inputs. In fact, any innate components of the organism have in these recent decades been customarily assumed to be aspects of either response organization (instinct) or capacity (intelligence). Freud (1938), of course, was an exception to this rule in that he gave a large place to the hedonic value, and presumably also to the reinforcement value, of stimulation of the erogenous zones. Yet, in so doing, he was inconsistent with his later formal theorizing about motivation, namely, with his assertion that pleasure is reduction in stimulation and excitation, and that the function of all behavior and mental activity is to abolish or to master stimulation and excitation (Freud, 1950, pp. 62–64).

McClelland, Atkinson, Clark, and Lowell (1953) provide another exception, but one which fails to agree with the results of these three experiments. In their attempt to synthesize Helson's (1947, 1948, 1959) adaptation level (AL) theory with Hebb's theorizing about cell assemblies and phase se-

quences, they assume that various receptor inputs have innate ALs, and that inputs which deviate to only a slight or moderate degree from the AL, native or acquired, will be pleasant while those deviating to a marked degree, and to a marked degree in either direction from the AL, will be unpleasant. This formulation of the discrepancy hypothesis predicts that positive and negative hedonic values, and presumably also reinforcement values, being a function of the discrepancy from the AL, should fall on a symmetrical "butterfly" curve as Haber (1958) has indicated (see Figure 1). This formulation of the discrepancy principle provides an interesting answer to the question concerning what it is that accounts for differential hedonic values for stimuli within what would otherwise be regarded as a neutral range. Moreover, Haber (1958) has shown that the prediction of a symmetrical butterfly curve holds for paired comparisons of the hedonic values of temperatures that deviate by a range of from 1 to 15 degrees from the AL, i.e., that temperature which has come through the adaptation that occurs with continued immersion of a part of the body in water of a given temperature till it yields "no temperature sensation." The symmetrical butterfly curve, however, was shown empirically only when the AL was made to approximate the skin temperature of each subject, and no effort was made to get both wings of the curve for ALs well above or well below skin temperature.

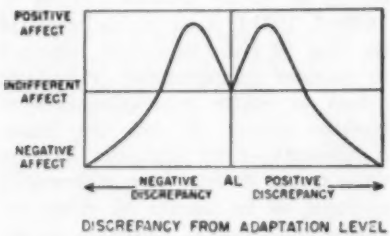

FIG. 1. A theoretical curve depicting the relation between affective value and discrepancy of level of stimulation from the adaptation level. (After Haber, 1958)

The study of vibration reported here and also the studies of the preferential reactions of animals reared on SOA and on flashing lights all indicate that the prediction of the symmetrical butterfly curve does not hold for those forms of stimuli which have innately negative reinforcement value. In these three studies, no attempt was made to test the preferential behavior or reinforcement values of slight discrepancies from the ALs established by being reared on the various experimental conditions of stimulation. Even so, the lack of symmetry in any curve that might have been gotten from such measurements is clearly indicated by the fact that the animals reared on the vibrating table, on SOA in the drinking water, or on flashing illumination preferred test conditions with absence of these accustomed forms of stimulation while animals reared on a still table, on ordinary water, or on constant illumination preferred test conditions with the presence of these accustomed forms of stimulation. Presumably the sizes of the discrepancies between the conditions of test and the AL established by the conditions of rearing are the same for both directions of change in stimulation for all these experiments, so a symmetrical butterfly curve would be impossible, and the asymmetry constitutes the evidence for innate reinforcement value.

Evidence for Innate Reinforcement Value from Other Sources

We come somewhat reluctantly to an espousal of this hypothesis of innate reinforcement values for receptor inputs. The concept is dissonant with our past theoretical prejudices. Never-

theless, the evidence from our present experiment and that from the experiments of Warren and Pfaffmann (1958) and of Meier et al. (1960) indicate that negative reinforcement value must be a genuine aspect of various kinds of receptor inputs. Evidence from several other sources also supports the hypothesis that various receptor inputs may have innately either positive or negative reinforcement values.

Pratt's (1954) review of the characteristics of the human neonate contains several bits of such evidence. For instance, if duration of an infant's fixated gaze is a valid indicator, the colors blue and green have greater positive value than does the color red, and all these have greater value than does yellow (Stirnimann, 1944, as cited in Pratt, 1954). Again, if turning away reflects negative value and sucking and licking reflect positive value, the odor of the oil of chenopodium has negative hedonic value, while the odors of anise oil, of the essence of lavender, and of valerian mint have positive value (Ciurlo, 1934, as cited in Pratt, 1954).

Even more convincing than such evidence, however, is that obtained by Harlow (1958) in his study of the origins of love in infant monkeys. In terms of the time spent by the infants on their padded mother surrogates as compared with the time spent on the wire mother surrogates on which they were fed, the quality of softness in padding appears to have more positive reinforcing value than does hunger gratification.

Finally, the fact that Olds (1955) and his collaborators (Olds & Milner, 1954; Olds & Olds, 1958) have found that both electrical and chemical stimulation of the septal area is positively reinforcing, while such stimulation of the medial lemniscus, which is believed to feed excitement to the brainstem reticular formation, is negatively reinforcing (see also Miller, 1957) suggests that certain centers exist within the brain where excitement is positively reinforcing and other centers where excitement is negatively reinforcing. Perhaps the fact that the septal area is part of the old olfactory brain which has been phylogenetically concerned with sex and consummatory activities may account for the positive reinforcing and hedonic value of various odors and flavors associated with sex and consummatory responses and also of the feedback from such activities.

At any rate, the notion of reinforcement value, and presumably also hedonic value, being innately associated with receptor inputs is of consequence in our theory of psychological reinforcement, and the evidence available is sufficiently supportive to indicate that the hypothesis of innate reinforcement values deserves serious investigative consideration.

SUMMARY

Hebb's (1949) theory of emotional arousal and disturbance asserts that when stimulation to which an organism has become accustomed is removed, emotional disturbance results which motivates escape and which renders the accustomed form of stimulation positive in reinforcement value.

This theory suggested an exploratory attempt to demonstrate the principle experimentally. Rats reared with vibration, from before birth to 56 days of age, did press a bar to stop vibration less often than did controls, but they did not, as expected from Hebb's formulation of the discrepancy principle, press a bar more often than did controls to start vibration. This finding, coupled with a similar finding for a bitter substance by Warren and Pfaffmann (1958), and for flashing illumination by Meier et al. (1960), does not

deny the discrepancy principle. Since merely becoming accustomed to stimulation which is innately negative in reinforcement or hedonic value does not alone render it positive in reinforcement value, however, these experiments suggest that some stimuli have innate reinforcement value which places limits upon the discrepancy principle beyond those already recognized in actual noxiousness or painfulness.

The notion of receptor inputs having innate reinforcement or hedonic value, it is pointed out, deserves further investigative consideration.

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(Received November 27, 1959)

Psychological Review
1961, Vol. 68, No. 3, 157–166

STATISTICAL THEORY OF PERSUASION

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Imperial Oil Limited, Toronto

Free social interaction has so far defied efforts to conceptualize it into a theory from which predictions could be made. As long as experimental conditions and variables were not sufficiently well specified to allow operational definition of the necessary concepts, it was perhaps premature to attempt construction of such a theory. For, in the absence of these conditions, interaction may be forced into inappropriate conceptual molds, which might disregard either its individual or its interactional aspects, with the consequence that predictions either cannot be made or cannot be tested. However, recent advances in experimental techniques and theoretical thinking in social psychology seem to indicate that the time is ripe for an attempt to formulate a limited, but testable, theory of free interaction (experimental persuasion between two individuals), with mathematically developed implications of basic assumptions.

The experimental arrangements for the study of social interaction can be roughly divided into two classes: (*a*) The arrangement allowing free behavior of one individual (or group) in a social situation which is under the control of the experimenter. This arrangement more or less duplicates that of individual psychology and, therefore, can be conceptualized in terms of systems similar to those proposed for individual behavior. (*b*) The arrangement allowing a free interaction between two or more given individuals in a given setting. Since in this situation each freely behaving individual represents part of the other individual's environment, an essential part of each individual's environment is not under the experimenter's control. This arrangement, peculiar to social psychology, is thus different from that used in individual psychology in an essential characteristic, and therefore cannot be handled by theories and methods of individual psychology without serious modification.

This paper proposes a conceptual and mathematical apparatus which can handle the problem of predicting (though not yet very precisely) free persuasive interaction between two individuals. It is also hoped that "individual" behavior in social situations, which appears to be a special case of free interaction, can be explained by this theory. In this case the behavior of the "other" individual can be brought under the direct control of the experimenter by using "confederates." The relation of the theory to models of minimal social interaction is more complicated (see below).

The application of the proposed models (at least two complementary models are required for prediction) will be illustrated by experimental examples of free persuasive interaction between two individuals (with pairs of subjects selected for desired combinations of their personality characteristics). In two of these experiments pairs of subjects discussed a topic on which they initially disagreed, with the purpose of reaching, whenever possible, a final agreement. In the third experiment the interac-

[1] Much of this work was done while the senior author was still at the University of Toronto.

tion was in the form of successive silent numerical bids for sale and purchase of a secondhand automobile, which were written down by the subjects alternately. Each subject's objective was to persuade the other subject, by the manner of his bidding, to accept his price.

GENERAL CONSIDERATIONS

The writers consider that scientific knowledge consists of tested propositions about the relations between independent (manipulable) and dependent (not directly manipulable) variables, whose variation depends to some extent on the variation in the independent variables. The validity of such propositions is tested by manipulation of independent variables, to see if the predicted changes in the dependent variable can actually be obtained. In social interaction, where not all the stimulus variables are under the experimenter's direct control, it is proposed to define independent and dependent variables in terms of experimenter's and subjects' control, rather than in terms of stimulus and response variables. This distinction is not important in the first type of experimental arrangement, because the stimuli responsible for changes in the subjects' behavior are under the experimenter's direct control. The distinction is important, however, in free interaction, because the "other" freely behaving subject supplies stimuli which may be responsible for changes in a subject's behavior, but are not under the experimenter's direct control.

We assume that at any particular moment (or trial) we have only one responding subject, the other subject, together with the rest of the environment being the source of stimuli. This allows the application of the operant paradigm, because we have to deal only with the response of one subject, the reinforcement (response of the other subject), and have no eliciting stimuli to worry about. Since, however, the situation changes from moment to moment, subjects' responses alternating constantly, all the possible operants will have to be taken into consideration with respect to time. From such an assumption, models of interaction can be derived by purely mathematical reasoning.

However, in order to make genuine predictions about interaction, it is necessary to be able to estimate the initial values of the operants and their rates of change, which may be either constant or changing throughout an experiment, from variables under the experimenter's control. Predictions of the initial responses can be made by simple extrapolation from previous responses to similar stimuli (for example, in a questionnaire). Prediction of the rate of response change can be derived from its regression function on subjects' personality or other variables under the experimenter's control.

MODELS OF PERSUASIVE INTERACTION BETWEEN TWO INDIVIDUALS

The models presented in this section are derived from the application of the operant paradigm to interaction. The variables suggested by the operant paradigm are: response, reinforcement, discriminative stimuli. In a pair of two interacting individuals the response of one individual can be operationally defined as the reinforcement of that of the other, and vice versa. Responses, therefore, represent the two dependent variables. The independent variable in interaction models is time. The discriminative, or controlling, stimuli can be operationally defined as the conditions of the experimental arrangement used

(instructions, topic discussed, etc.). From the point of view of interaction models, these are constant, although they will be treated as experimental variables from the point of view of persuasibility models (see next section).

Definition of Response

Following Skinner and Estes, it is assumed that verbal statements made during a discussion experiment, or numerical responses in a bidding game experiment, are independent samples of one of two classes of responses,[2] that they can be so classified, and that probability measures can be associated with each of these classes. Sequences of such responses are considered to represent behavior of interacting subjects.

This definition may be clarified by an example. In a verbal interaction experiment, two subjects are selected who initially disagree on a topic, such as desirability of national health insurance, desirability of changing divorce laws, and so on. These two subjects are instructed to discuss the topic and come to some conclusion, agreeing on one or the other opinion, compromising, or disagreeing. At the signal the two subjects start discussing the topic; the time lengths of their responses are continually registered on a multiple-pen recorder, and simultaneously every statement is classified in one of two classes: positive opinion on the topic (say, divorce laws in Canada should be changed) or negative opinion (divorce laws in Canada should not be changed). Agreeing and disagreeing statements operationally define the two classes of responses. Each statement is a sample of one of these two classes. A closed system is postulated in the sense that all responses used are classified in one or the other of the response classes. Only two-subject situations are considered, so that altogether there are four categories of responses.

The ratio of the length of positive statements to the total length of a subject's speaking during an interval is taken as an estimate of the momentary probability of a subject holding a positive opinion on the topic. These probabilities may change during the discussion. The function of the model is to describe the change in the probability of each subject's response during the discussion, and estimate its final or asymptotic probability, given its initial probability.

Derivation of the Models

The system or universe (R) to be considered consists of two subuniverses (R_1 and R_2) of responses (or repertoires of arguments—Skinner, 1957) to a certain topic, one for each subject. Each subject's subuniverse is divided into two classes (R_{i1} and R_{i2}) of responses corresponding to affirmative and negative opinions on the topic. Some of these responses were sampled by each subject in his previous life and are readily available as arguments for discussion. Others are present only potentially. Those responses that already have been sampled and reinforced constitute the subject's opinion which he brings with him into the laboratory (cf. transfer of learning). In an experiment, responses are sampled at some constant rate, presumably first those that constitute the subject's opinion and then perhaps new ones. The whole process of sampling of responses is assumed to be similar to that postulated by Estes (1950) for stimuli.

The occurrence of two samples of

[2] Neutral statements were recorded during the experiment, but they were excluded from further calculations.

response of the same class in one time interval is assumed to increase, and the occurrence of responses of different classes to decrease, the probability of future occurrence of the first response, cf. Bush & Mosteller, 1955. The closed system assumption implies that when the probability of one response increases, that of the other decreases by the same amount. The following notation will be used:

R_1 = response repertoire of S_1 with measure $p_1 = R_1/R$.

R_2 = response repertoire of S_2 with measure $p_2 = R_2/R$.

where:

$$p_1 + p_2 = 1 \qquad [1]$$

and R is the total pool of responses of the two subjects, so that $R_1 + R_2 = R$.

R_{11} = response repertoire of S_1 in Class 1 with measure $r_{11} = R_{11}/R$.

R_{21} = response repertoire of S_2 in Class 1 with measure $r_{21} = R_{21}/R$.

etc.

A_{ij} = number of arguments for the ith opinion sampled and learned (by S_i) in an interval from the as yet unreinforced (or to be learned) part of the subject's repertoire while listening to S_j with sampling measure $a_{ij} = A_{ij}/R$.

B_{ij} = number of arguments sampled and unlearned (by S_i) in an interval from the already reinforced (or to be extinguished) part of the subject's repertoire while listening to S_j with sampling measure $b_{ij} = B_{ij}/R$.

Moreover $R_{i1} + R_{i2} = R_i$ and:

$$p_i = r_{i1} + r_{i2} \qquad [2]$$

Further we define:

$$p_{ij} = r_{ij}/p_i \qquad [3]$$

so that:

$$p_{i1} + p_{i2} = 1 \qquad [4]$$

The interpretation of p_{ij}, r_{ij}, and p_i is this: p_{ij} is the estimate of probability that ith individual *holds* jth opinion; r_{ij} is the estimate of the compound probability that ith individual holds jth opinion and will actually *voice* it; p_i is the estimate of probability that ith subject will *speak* (say something).[3]

Model 1. A general model of persuasive interaction can be written as an average rate of increase in probabilities of, say, p_{11} and p_{21}. Both self- and alter-reinforcement is allowed. The proportions (a and b) of operants sampled out of the repertoire in each interval will be assumed to be constant throughout the experiment. With these assumptions we have for p_{11} a set of eight reinforcement probabilities and eight possible values of rates of increase[4] of response probabilities based on different sampling rates:

Reinforcement Probabilities for R_{11}	Possible Values of $dp_{11}/dt = \dot{p}_{11}$
$r_{11}r_{11}$	$a_{11}(1 - p_{11})$
$r_{11}r_{12}$	$-b_{11}p_{11}$
$r_{11}r_{21}$	$a_{12}(1 - p_{11})$
$r_{11}r_{22}$	$-b_{12}p_{11}$
$r_{12}r_{11}$	$a_{21}(1 - p_{11})$
$r_{12}r_{12}$	$-b_{21}p_{11}$
$r_{12}r_{21}$	$a_{22}(1 - p_{11})$
$r_{12}r_{22}$	$-b_{22}p_{11}$

Multiplying the two columns term by term and summing we obtain the average rate of increase of p_{11}:

$$\dot{p}_{11} = (1 - p_{11}) \sum_i \sum_j a_{ij} r_{1i} r_{j1} - p_{11} \sum_i \sum_j b_{ij} r_{1i} r_{j2}$$

[3] Probabilities p_i were constant throughout the experiment with some variation around the mean, but no systematic changes.

[4] The concept of the universe of the subject's repertoire was used in defining the rate (a, b) of the sampling of arguments. The concept of response classes, on the other hand, was used in defining the probabilities of reinforcement.

Using Equation 3, we get an equation of the form:

$$\dot{p}_{11} = Ap_{11} + Bp_{21} + Cp^2_{11} + Dp_{11}p_{21} + Ep^3_{11} + Fp^2_{11}p_{21} \quad [5]$$

The expression for $\dot{p}_{21}$ is similar.

This system of differential equations describes the average change in probabilities of our subjects holding Opinion 1. It can be solved numerically to obtain curves fitting the experimental data of interaction.

It is obvious that in this form the model is not very useful for predicting behavior because of the large number of parameters that would have to be estimated from previous behavior of subjects and their personality variables. However, we can simplify the model by introducing additional assumptions.

Model 2. A useful simplification can be obtained by putting:

$$a_{ij} = b_{ij} \qquad a_{12} = a_{22} \qquad \text{(for } S_1\text{)}$$
$$a'_{ij} = b'_{ij} \qquad a'_{21} = a'_{11} \qquad \text{(for } S_2\text{)}$$

Equation 5 and the corresponding equation for $\dot{p}_{21}$ then simplify to:

$$\dot{p}_{11} = b_1(p_{21} - p_{11}) \quad [6a]$$

$$\dot{p}_{21} = -b_2(p_{21} - p_{11}) \quad [6b][5]$$

where:

$$b_1 = b_{12}p_1p_2$$
$$b_2 = b_{21}p_1p_2$$

These equations on integration give:

$$p_{11} = \frac{b_2p^0_{11} + b_1p^0_{21}}{b_1 + b_2} + \frac{b_1(p^0_{11} - p^0_{21})}{b_1 + b_2} \times \exp[-(b_1 + b_2)t] \quad [7a]$$

$$p_{21} = \frac{b_2p^0_{11} + b_1p^0_{21}}{b_1 + b_2} + \frac{b_2(p^0_{21} - p^0_{11})}{b_1 + b_2} \times \exp[-(b_1 + b_2)t] \quad [7b]$$

[5] Equation 6b was derived by symmetry. The b-coefficients with one subscript in Equation 6 are not to be confused with the b-coefficients with two subscripts used previously.

where p^0_{11}, p^0_{21} are initial values of p_{11} and p_{21}. As $t \to \infty$, p_{11} and p_{21} approach a common limit:

$$L = \frac{b_2p^0_{11} + b_1p^0_{21}}{b_1 + b_2} \quad [8]$$

There are only three types of parameters in these models:

1. Initial value of response probabilities (p^0_{ij})
2. Average values of participation probabilities (p_i)
3. Parameters of opinion change (b_{ij}) which will be called coefficients of persuasibility. In the next section these coefficients will serve as behavior measures. In Model 1 different coefficients for each operant are assumed. In Model 2 only one general coefficient of persuasibility appears for each subject.

Application of Model 2

A basic condition for usefulness of a model is that it should describe the experimental data closely enough to be considered a worthy substitute for them. Model 2 was fitted to experimental data, to see if it could describe the change of opinions and their asymptotic values closely enough. The data were obtained from three experiments to be described elsewhere.

Two of these factorial experiments (Cervin & Carment, 1960) were on verbal persuasion between two subjects and were run under conditions identical to those used in previous studies (e.g., Cervin, 1957), except for personality variables used in the selection of pairs. The third experiment (Cervin, Joyner, & Bregmann, 1960) was similarly designed, but the task was a silent bidding game. Each pair of empirical curves of response probability obtained in these experiments was fitted with the func-

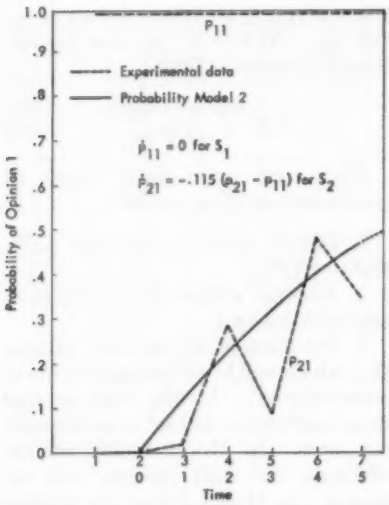

FIG. 1. Course of discussion ending in S_2 changing his opinion (case of persuasion).

tions of Model 2.[6] A random sample of pairs of empirical and fitted curves from Experiment 1 and Experiment 3 is given in Figures 1 to 4. Empirical probabilities (p_{ij}) of verbal responses were estimated by taking the proportion of time S_1 spoke, say, in favor of Opinion 1, to the total time he spoke in an interval. Numerical bids in the bidding game experiment were taken without transformation into probabilities, but this could be easily done by dividing each bid by the maximum bid. The results would be the same except for absolute numbers.

Model 2 reasonably well describes the middle and end phases of the discussions, from the moment when subjects' positions begin to shift to the moment when they reach new levels of agreement, but does not apply to the initial phases, when subjects' opinions are predefined and remain constant. In the initial phases of the discussions we probably have the transfer of opinions from previous situations. The models of change of opinion perhaps should not be expected to describe the transfer phase, except by a constant. On the other hand, in the bidding game the responses are described adequately over the whole length of the experiment, probably because, owing to different instructions, the change started right from the beginning of interaction.

[6] Curve fitting was done by the method of linear regression of the to be fitted model values on the integral of the experimental data. The method was suggested by G. S. Watson of the Department of Mathematics, University of Toronto.

INDEPENDENT ESTIMATION OF PERSUASIBILITY COEFFICIENTS

As pointed out earlier in this paper, some of the variables in free persuasion experiments, e.g., response controlling stimuli, can be controlled by the experimenter directly by means of instructions, tasks, etc.; others how-

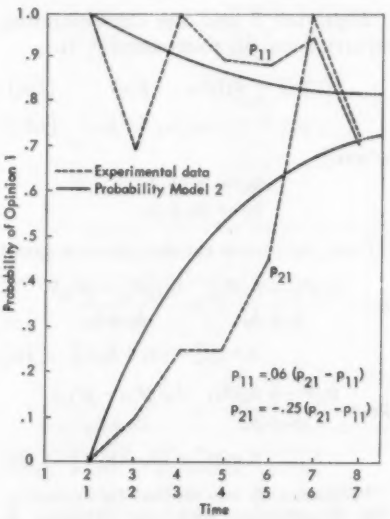

FIG. 2. Course of discussion ending in both subjects changing their opinions, but S_2 yielding more than S_1 (partial persuasion).

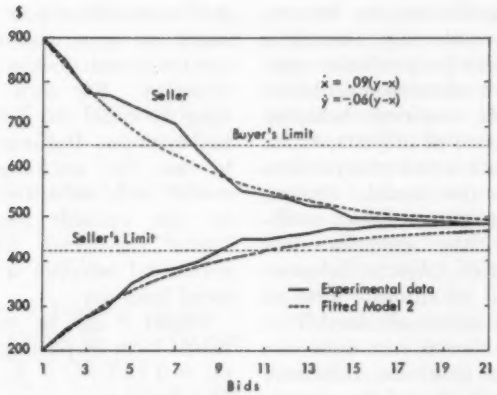

FIG. 3. Course of bidding ending in seller gaining a larger portion of the price difference.

ever, (e.g., reinforcement schedules) can be controlled only indirectly by means of selection of subjects with certain personality scores on the assumption that subjects with such scores may be expected to produce certain responses, i.e., reinforcement schedules.

When the experimenter controls his independent variables directly, he can obtain independent measures of these variables, as well as those of dependent (i.e., subjects' behavior) variables. If his model relates dependent variables to the independent ones, as models usually do, he can compare his experimental measures with his predictions directly.

But when the experimenter does not control the experimental variables directly, as in the case of reinforcement in free persuasion, he can obtain no independent measures of them. Therefore, in our interaction model, we introduced time as a dummy independent variable and, following Hull (1951) and Cervin (1957), certain personality scores as the real independent variables, varied by the experimenter by means of selection of subjects. Since our interaction model relates the dependent variable of each

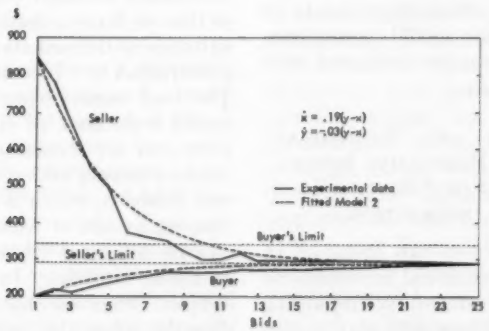

FIG. 4. Course of bidding ending in seller gaining the whole of the price difference.

discussion situation to the dummy independent variable only, alone it is clearly insufficient for prediction purposes. But each discussion situation gives a pair of empirical behavior curves for each pair of subjects, which can be fitted with a pair of equations from our interaction model. Parameter values (e.g., persuasibility coefficients) of the fitted equations can serve as indices of subjects' behavior in experimental situations. Sets of such parameter values obtained from a set of experimental situations can be regarded as (subjects' behavior) variables dependent on the experimental variables controlled by the experimenter. Some hypotheses concerning the nature of this dependence (e.g., Hull, 1951; Cervin, 1957) have been tested by means of regression analysis of these parameter values on personality scores (Cervin & Carment, 1960; Cervin et al., 1960). These experiments so far have produced significant regression equations with multiple *R*s of up to .47. For practical prediction we need, of course, more precise regression equations, but this is merely a matter of more experimental labor.

Regression analysis has been chosen because, besides the tests of significance of the hypothesized relations, it also gives estimating equations which can be used in other experiments to predict interaction model parameters, which, in turn, can be compared with experimental data.

Some Models of "Individual" Behavior in Persuasive Interaction and of "Minimal" Social Interaction

The experimenter can bring all of an individual's external environment under his direct control by employing confederates. These will play a prescribed role and thereby provide the desired reinforcement to the subject much as apparatus or the experimenter himself does in an "individual" situation. For such "social" situations we could use individual conceptual systems. But it is more satisfying to use the unchanged interaction model, only substituting a constant for the variable responses of the "other" individual. This would make individual behavior a special case of social behavior.

Model 2 can be reduced to individual form by putting $b_2 = 0$. Then $\dot{p}_{21} = 0$ and $p^0_{21} = p_{21}$ and Equation 7a reduces to:

$$p_{11} = p^0_{21} - (p^0_{21} - p^0_{11})e^{-b_1 t}$$

For any constant p_{21}, as $t \to \infty$, $p_{11} \to p^0_{21}$, that is the proportion of time S_2 reinforces Opinion 1 of S_1. This equation is identical in form with Estes' (1950) Equations 3 and 10 for partial reinforcement. Thus an individual model of learning appears as a special case of the more general interactional model.

In the so-called "minimal" social interaction (Burke, 1959) the probability of reinforcement is a function of the response probabilities of the two interacting subjects and the payoff matrix of the experimenter (π, p, p'). The experimenter is interposed between the two subjects, so that we have a double "individual" situation as defined above, rather than a simplified free interaction situation. The four events from which Burke's model is derived all involve two subjects and are formally identical with the four events underlying our simplified Model 3, which is not given here. The equations of Model 2, however, can be obtained from a special case of Burke's model. In Model 2 each subject receives his reinforcement directly from the other subject instead of through the experimenter.

In order to satisfy this condition, Burke's probabilities π_{ij} have to have the values:

$$\pi_{11} = \pi'_{11} = \pi_{21} = \pi'_{21} = 1$$
$$\pi_{12} = \pi'_{12} = \pi_{22} = \pi'_{22} = 0$$

and Burke's Equations 9 become:

$$p_{n+1} - p_n = \theta(p_n - p'_n)$$
$$p'_{n+1} - p'_n = \theta'(p_n - p'_n)$$

Letting the time interval approach zero, our Equations 6a and 6b are obtained from these equations.

Although Burke's and our models were constructed to describe different experimental situations, they can be both specialized to describe the same abstract situation whose essential features can be reproduced in both his and our experimental arrangements. When this is done, the two models agree.

The more general form of Burke's model allows the experimenter to intervene by specifying the π's which partially determine the reinforcement. But it does not take self-reinforcement into account. Our general Model 1 includes self-reinforcement but not the experimenter's intervention, except in its individual form. There may therefore be other ways of relating the two models.

DISCUSSION

The preceding sections give models of free social interaction and propose a method of arranging experiments and estimating parameters from variables under the experimenter's control, i.e., independently of the data described by these models. The interaction model has been validated so far on data from two verbal and one nonverbal experiments, which will be reported in separate papers. In their present form the persuasibility models serve as illustrations of the proposed relation between theory and experiment in social interaction, rather than as practical prediction guides to be used. However, in view of the considerable amount of experimental work which preceded (and followed) the conception of the theory, some confidence in the models is justified. Eventual practical use of persuasibility models is illustrated by two examples.

The relation of the proposed theory to other theoretical work has been touched upon. It is doubtful, though, that it can be usefully compared with theoretical work in social psychology proper. Only theories with similar scientific goals can be usefully compared or related. For example, two theories which try to predict behavior can be compared. But if one theory tries to predict behavior from objectively measurable conditions, and another to infer the phenomenological field of an individual from his behavior (and sometimes also this behavior back from his phenomenological field), nothing fruitful can be derived from their confrontation.

It may be useful, however, to comment on the relation of the assumptions in the present essay to those underlying some other theories. The theory presented is built mostly with elements borrowed from other psychologists. Thus the conception of interaction as a closed system is akin to Lewin's field concept (here operationally defined by the system of simultaneous differential equations), although Estes' influence was more direct. The statistical concept of response was borrowed from Skinner (1957) and Estes (1950). The idea of sampling responses (arguments) was borrowed by analogy from Estes' idea of sampling of stimuli. The operant paradigm used is, of course,

due to Skinner, although Konorski and Miller's paradigm of voluntary movements is appropriate too, because in the experimental situations an eliciting stimulus was used to start the discussions.

In spite of this indebtedness, it is possible that none of the authors cited would agree with the final product of the writers' labors. For example, most of the authors cited are stimulus-response theorists, whereas it is only with some qualifications that a stimulus-response label can be attached to the present effort. Moreover, present-day social psychology is still strongly antibehavioral, while our theory of interaction is thoroughly behavioristic and stands or falls with the definition of social psychology as a behavioral science. However, we hope that this essay will clarify some of the theoretical issues of social interaction and suggest an avenue to more rigorous treatment of this difficult area.

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(Early publication received August 23, 1960)

Psychological Review
1961, Vol. 68, No. 3, 167–177

PSYCHOLOGICAL MODELS FOR RELATING DISCRIMINATION AND MAGNITUDE ESTIMATION SCALES

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Educational Testing Service

Two broad classes of psychophysical scaling techniques, ratio methods and category methods, have been used to describe several psychological dimensions. When these methods are compared on the same perceptual continua, they are frequently found to be curvilinearly related (Stevens & Galanter, 1957). This curvilinearity is usually attributed to systematic distortions introduced by one or both procedures, otherwise there would be more than one psychophysical law. The present paper attempts a different solution to this problem of curvilinearity by presenting a psychological rationale for explaining differences between such scaling procedures and a method of analysis for rectifying obtained curvilinearities.

Psychophysics is concerned with the specification of relationships between at least two measurement scales: a physical scale, usually with an empirically testable measurement theory, and a sensation scale, which sometimes has features of its associated measurement scale unverified. In order to evaluate the validity of various approaches to the scaling of sensations, it seems necessary to consider empirically derived relationships between various psychological scales, as Stevens (1957) and Stevens and Galanter (1957) have done. It seems equally important, however, to consider the implications of various scaling models and of the psychological notions underlying these models for the relations between scales.

Luce (1959b) has attempted to bring a measure of order into the discussion of psychophysical relationships by proposing some ground rules for the construction of psychophysical theories. Given his particular theoretical principles, the number of possible relations between measurement scales is severely limited. In contrast to Luce's purely mathematical development, however, the present paper will explore some of the consequences *for the relations between scales* of the psychological notions presumably underlying certain scaling models. Some relevant data derived from multidimensional scaling experiments will also be considered. The paper will be concerned primarily with the comparison of direct ratio scales and indirect Thurstone scales and in particular with the relationship between magnitude estimation scales and discrimination scales, such as those derived from paired comparison or successive intervals procedures (cf. Torgerson, 1958). The latter successive intervals scales, resulting from an application of the Law of Categorical Judgment (Torgerson, 1958) to category data, should be distinguished in the present discussion from partition scales, which arise from a simple averaging of category ratings (Stevens, 1960).

[1] The authors gratefully acknowledge the stimulating comments received during the consideration of this problem from Robert Abelson, Eugene Galanter, Harold Gulliksen, and S. Smith Stevens.

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DIRECT AND INDIRECT LINEAR SCALING MODELS

The direct quantitative estimation of psychological magnitudes (Stevens, 1956) and the direct determination of ratio relationships (Stevens, 1958) have provided a basis for scaling methods in psychophysics for a wide variety of sensory continua. The psychological models underlying this direct approach may be characterized as deterministic in the sense that each subject is presumed to have a fixed "real" scale of judgment and his verbal reports are presumed to reflect "real" metric properties (Messick & Abelson, 1957). In a particular method (Stevens, 1956), for example, it is assumed that each subject assigns a number to a stimulus in proportion to the ratio which that stimulus bears to a fixed standard on his perceptual scale. Equal stimulus ratios are assumed to produce equal sensation ratios, and this relationship between a physical and a sensation metric may be expressed as a power function, $\psi = kS^n$, where ψ represents the value of the sensation and S represents the amount of the stimulus.

The indirect approach to psychophysical measurement advocated by Fechner and Thurstone differs considerably from the direct methods in that scales are developed using judgmental variability in responding to imperfectly discriminated stimuli. Among the basic statistics from which these indirect scales may be derived is the proportion of times each stimulus is placed in one of a series of ordered categories (as in the method of equal-appearing intervals and successive intervals), or the proportion of times each stimulus is judged to exceed a standard (as in classical psychophysics), or to exceed each other stimulus (as in the method of paired comparisons) (Guilford, 1954; Torgerson, 1958). The task is specified in terms of some particular attribute and involves such judgments as "greater than," "brighter than," etc. The basic proportions used in generating these scales are considered to be estimates of probabilities, and hence the underlying models may be considered stochastic: each individual is taken to act as though he refers perceptions to scales of extent or degree, and his momentary impressions are subject to random variations on these scales. Properties of these stimulus confusions lead to the development of scales of measurement.

Psychophysicists in the Fechnerian tradition usually assumed that just noticeable differences were equal throughout the stimulus range and that the addition of units thus defined provided a subjective scale (Luce & Edwards, 1958). The proportion of times, p_{ab}, that stimulus a was judged greater than stimulus b could be thought of as a monotonic increasing function of differences in their respective scale values, s_a and s_b:

$$p_{ab} = f(s_a - s_b) \qquad [1]$$

Thurstone recognized that equally often noticed differences were not necessarily equal throughout the stimulus scale (Thurstone, 1927b, 1927c) and modified Fechner's position to form the Law of Comparative Judgment (Thurstone, 1927a):

$$p_{ab} = f\left(\frac{s_a - s_b}{\sqrt{\sigma_a^2 + \sigma_b^2 - 2r_{ab}\sigma_a\sigma_b}}\right) \qquad [2]$$

where s_a, the scale value of stimulus a, is the mean of a distribution of judgment processes associated with the stimulus; σ_a, the discriminal dispersion, is the standard deviation of the distribution of judgment processes; and r_{ab} is the correlation between judgment processes for two stimuli.

Thurstone assumed the function relating proportions to differences in scale values to be a normal ogive, and the law is most frequently stated in that form:

$$s_a - s_b = z_{ab}\sqrt{\sigma_a^2 + \sigma_b^2 - 2r_{ab}\sigma_a\sigma_b} \quad [3]$$

where z_{ab} is a normal deviate value corresponding to p_{ab}. The simplest and most widely applied form of the Law of Comparative Judgment is Case V (Mosteller, 1951a; Thurstone, 1927a), which assumes discriminal dispersions to be equal for all stimuli and the correlations, r_{ab}, to be constant. With $\sigma\sqrt{2(1-r)}$ taken as a unit, Case V may be stated as:

$$z_{ab} = s_a - s_b \quad [4]$$

which is identical to the Fechnerian position given in Equation 1, if the function f is specified to be cumulative normal. However, in the more complicated Case III of the Law of Comparative Judgment (Thurstone, 1927a), discriminal dispersions are permitted to vary throughout the scale, thus allowing the condition emphasized by Stevens (1957) that dispersions increase as the subjective scale value increases. Since available solutions to Case III are impractical, Case V is most frequently used to analyze data gathered by the method of paired comparisons, and hence inaccuracies would be introduced into the scaling of prothetic continua (Stevens, 1957), if variability actually increases as subjective intensity increases. However, practical solutions allowing unequal dispersions do exist for the method of successive intervals (Diederich, Messick, & Tucker, 1957; Gulliksen, 1954; Torgerson, 1958).

MULTIDIMENSIONAL SCALING

Multidimensional scaling methods are based upon the assumption that properties of psychological distance are analogous to properties of physical distance and may be analyzed in terms of the mathematical procedures of distance geometry. In these methods, psychological distances are usually derived from judgments of similarity among stimuli: i.e., two stimuli judged to be very similar are considered to be separated by a smaller psychological distance than two stimuli judged to be very different. In contrast to linear scaling procedures, wherein judgments are made with regard to a particular attribute, the multidimensional scaling judgment requires the subject to operate at a possibly more primitive level where total similarity with respect to a general domain prescribes the perceived relation between stimuli. Given judgments of similarity among all the stimuli in a set, it is necessary to deduce the number and nature of the scales which are sufficient to describe the reported relations among the stimuli. Intrinsic to the method, then, is the possibility of relating two or more linear scales within a common structure—provided, of course, that general similarity between two stimuli may be considered a measurable attribute in the same sense that brightness and loudness are measurable.

Various distance geometries may serve as models for relating the observed distances to the underlying psychological constructs. There appears to be strong justification for the utilization of Euclidean geometry for this purpose, at least in terms of the cohesiveness and intelligibility of a relatively large body of experimental evidence that has been analyzed in terms of Euclidean geometry (cf. Torgerson, 1958). In this approach the stimuli are treated as points in a Euclidean space, and analytical techniques are available to obtain the dimensionality of the space as well

as stimulus scale values determined within a rotation and translation (Messick, 1956c; Torgerson, 1952).

RELATIONS BETWEEN LINEAR SCALES

Stevens and Galanter (1957) have assembled psychophysical data comparing category scales and ratio scales on several different perceptual continua. On prothetic continua, where the fineness of discrimination varies from one end of the stimulus range to the other, category scales and ratio scales were found to be nonlinearly related. On metathetic continua, which involve a general uniformity of discrimination throughout the range, category and ratio scales were frequently found to be linearly related. Since none of the category scale analyses included in their survey permitted unequal discriminal dispersions, the nonlinearity on prothetic continua may have been due to distortions introduced into the category scale analysis by ignoring variations in subjective category widths and in stimulus dispersions (Messick & Abelson, 1957). Galanter and Messick (1961) subsequently found, however, that scale values derived from a successive intervals analysis of category data were even more curvilinearly related to magnitude estimation values for the same stimuli than were the standard category values. Present evidence thus suggests that corrections for stimulus dispersions and interval widths in category scaling will not account for obtained discrepancies between category and ratio scales and that some other rationale is required to explain the relationship.

An Equiprecision Model

Suppose subjects prefer to make discriminations on a scale with equal precision of judgment for all stimuli. Intuitively, it seems reasonable that an error in discrimination at one end of a scale should be no more undesirable to the subject than an error at the other end of the scale, at least in the laboratory. Precision of judgment may be defined as follows. Assume that the proportion of times stimulus a is judged greater than stimulus b is some continuous monotonic function of the two stimulus scale values:

$$p_{ab} = f(s_a, s_b) \qquad [5]$$

The partial derivative of p_{ab} with respect to s_a where s_a equals s_b is a value which will be denoted u_b:

$$u_b = \frac{\partial p_{ab}}{\partial s_a} \quad (\text{at } s_a = s_b) \qquad [6]$$

Thus u_b is a function of s_b. Note that when u_b is large, the change in p_{ab} as s_a is varied is also large, and the judgment of $a > b$ is relatively precise. When u_b is small (approaching zero), relatively large changes in s_a are accompanied by small changes in p_{ab}, and the judgment $a > b$ is relatively imprecise. The precision of judgments, then, may be defined by u_b.

Parenthetically, it can be pointed out that precision of judgment as here defined must be inversely related to the discriminal dispersions of the stimuli involved. Observe that the rate of change of overlap of the two discriminal distributions (which is what the definition of precision amounts to in the Thurstone model) as a function of change in difference between scale values is smaller for broadly spread distributions than for relatively peaked distributions.

Now consider what the result would be in applying this equal precision notion to judgments on a prothetic continuum, where fineness of discrimination varies throughout the stimulus range. Stevens (1957, 1960) has pointed out that for such con-

tinua the subject is relatively sensitive to small differences at the low end of the scale, whereas at the high end of the scale he is relatively insensitive. This implies that discriminal dispersions increase as scale values increase, if r_{ab} remains constant. Accordingly, for such a scale the precision of judgment would be inversely proportional to the scale value of the stimulus:

$$u_b = k\frac{1}{s_b} \qquad [7]$$

Now u_b, being the derivative of a function with cumulative distribution properties, may be considered to be the ordinate, F, of the discriminal frequency distribution for stimulus b at the point s_b:

$$u_b = \mathrm{F}(s_b) \qquad [8]$$

Consider the possibility of transforming the s scale to a y scale such that the precision of judgment for this new measure was a constant for all values on the scale. Let u_b' be the precision on the y scale defined similarly to u_b in Equation 6, substituting y_a and y_b for s_a and s_b, and let $\mathrm{F}'(y_b)$ be the ordinate of the y scale discriminal frequency distribution for stimulus b at the point y_b. For the y scale to have the property of equiprecision

$$u_b' = \mathrm{F}'(y_b) = C \qquad [9]$$

where the equivalence of u_b' and $\mathrm{F}'(y_b)$ is established in the same way as the equivalence of u_b and $\mathrm{F}(s_b)$ in Equation 8. Since the y scale is a transformation of the s scale, the relation between $F(s_b)$ and $F'(y_b)$ is given by

$$\mathrm{F}'(y_b) = \frac{ds_b}{dy_b}\mathrm{F}(s_b) \qquad [10]$$

Substituting into Equation 10 the appropriate values from Equations 7, 8, and 9 we obtain

$$C = \frac{ds_b}{dy_b}u_b = \frac{ds_b}{dy_b}k\frac{1}{s_b} \qquad [11]$$

which yields

$$y_b = \frac{k}{c}\log s_b + K \qquad [12]$$

where K is a constant of integration.

It is thus seen that a scale with constant precision of judgment will be produced by a logarithmic transformation of a psychological scale along which discriminal dispersions increase as a function of the stimulus value. On the other hand, if a subject's judgments were originally made with equal precision, then an antilog transformation would be required to recover the psychological scale with unequal dispersions. If it is assumed that subjects prefer to make judgments with equal precision, then on prothetic continua, where fineness of discrimination varies throughout the range, the derived equal precision scale will be a logarithmic function of the unequal dispersion scale. Thus an antilog transformation should be made on category scale values obtained from equal precision judgments before these values are compared with the corresponding magnitude estimation scale, where judgments were made directly in terms of ratio estimations. On metathetic continua, where discrimination is uniform, category and ratio scales should be directly comparable, as experimental evidence has indicated (Stevens & Galanter, 1957).

It should be emphasized that this equiprecision notion involves more than a mathematical juggling of existing mathematically defined relations. It involves a psychological principle—the utility of equal precision judgments in a discrimination situation—by means of which the theoretical relationship between two

different linear scaling models can be specified and obtained empirical relationships rectified.

The above development suggests the possibility that techniques which take account of judgmental variability may in some cases yield logarithmic interval scales instead of linear interval scales (cf. Stevens 1957, 1958). Rationales involving psychological principles other than the equal precision notion may also be offered for scales with such logarithmic properties. For example, suppose p_{ab} is a function of a ratio of scale values (v_a/v_b). If the Thurstone model is applied directly, the treatment of the probabilities as differences $(s_a - s_b)$ introduces a logarithmic transformation; i.e., if p_{ab} represents a ratio and it is treated as a difference in analysis, the scale values are implicitly transformed so that $\log v_a = s_a$. This would imply that an exponential transformation, allowing a free constant in the exponent, would relate the s scale based on differences to the v scale based on ratios.

For either rationale, then, an exponential transformation of the form

$$v_a = e^{ks_a+c} \qquad [13]$$

can be made on the discrimination scale values to recover a scale comparable to the direct magnitude estimation scale. The linear transformation of s_a was included in the exponent, since, if the Thurstone model fits, the scale values s_a are invariant up to a linear transformation (Adams & Messick, 1958; Gulliksen, 1946; Mosteller, 1951b). Equation 13 can be rewritten as:

$$v_a = (e^c)e^{ks_a} = ge^{ks_a} \qquad [14]$$

where g is defined to be the constant e^c.

Some stronger statements may be made about the relationship between the ratio judgment formulation and the difference judgment methods if a stochastic ratio model is considered instead of the deterministic model underlying Stevens' direct approach. Luce (1959a) and Bradley and Terry (1952) have independently suggested scaling models in which the proportion of times a is preferred to b is the following function of a ratio of scale values:

$$p_{ab} = \frac{v_a}{v_a + v_b} \qquad [15]$$

Equations 1 and 15 may be combined to relate the simplified difference model to this ratio approach:

$$\frac{v_a}{v_a + v_b} = \mathrm{f}(s_a - s_b) \qquad [16]$$

where $v_a = \psi(s_a)$. The problem is to determine the form of f whereby Equation 16 holds, as well as the form of ψ relating s_a and v_a. Bradley (1953) has shown that if the logistic curve is substituted for the normal ogive in the Thurstone model, then $\log v_a = s_a$; Luce (1959a) has proven that if s_a is set equal to $\log v_a$, then the distribution function for p_{ab} is logistic. Adams and Messick (1957) have shown that the only monotonic increasing function satisfying Equation 16 is the logistic, and the attendant relationship between s_a and v_a is given by

$$s_a = k \log v_a + C \qquad [17]$$

Since the logistic curve and the normal ogive are indistinguishable in practice, Equation 17 approximately relates the Thurstone approach based on differences to the stochastic ratio formulation.

Several other possibilities exist beyond these, however, for interpreting the observed relation between ratio and discrimination scales:

1. A model in which p_{ab} is considered to be a function of a ratio of

scale values (v_a/v_b) is a simple transformation of one in which p_{ab} is related to differences in logarithms ($\log v_a - \log v_b$). Thus, a psychological interpretation that subjects are actually judging ratios is indistinguishable from a claim of difference judgments on a log scale.

2. A model for direct estimation methods might be suggested in which the quantitative estimate of the psychological magnitude is considered to reflect a difference in scale values rather than a ratio. Analysis of these responses by the usual ratio assumptions would then require a log transformation to return to the difference scale specified by the model. In this way, one might take logs of ratio scale values in comparing the two approaches instead of taking antilogs of the Thurstone values.

3. The two types of scaling procedures generate different kinds of scales, perhaps tapping different sensory processes (Luce & Edwards, 1958). In this case, both kinds of scales would be useful, but for different purposes. Torgerson (1960) has presented some compelling arguments and data supporting the integrity of both methods. For example, he reversed the direction of scaling for both a category scaling method and magnitude estimation, obtaining independent judgments of both lightness and darkness of gray patches. As would be expected if category methods reflect difference judgments and ratio methods reflect ratio judgments, the lightness scale was the reverse of darkness for the category method and the reciprocal of darkness for the magnitude method.

The next section presents evidence that in some psychophysical situations a preference can be stated among the above positions and describes additional criteria for comparing scales.

MULTIDIMENSIONAL SCALING EVIDENCE

One procedure for obtaining psychological distances between pairs of stimuli for a multidimensional analysis is to apply traditional unidimensional scaling methods to all pairs of stimuli in a set and to interpret the obtained scale value for each pair as the distance between the members of the pair (Messick, 1956c; Young & Householder, 1941). Multidimensional extensions of paired comparisons and successive intervals have been applied in this way to judgments of similarity among colors (Mellinger, 1956b; Messick, 1956a; Richardson, 1938; Torgerson, 1951), friendship of nations (Klingberg, 1941), attitude relationships (Abelson, 1954; Messick, 1956b), and personalities (Jackson, Messick, & Solley, 1957). Since the unidimensional techniques discussed above have multidimensional analogues when applied to pairs of stimuli, the comparison of ratio and discrimination methods is also of interest in the multidimensional case. When treated multidimensionally, the question of whether data collected with a given scaling procedure should be analyzed in terms of a difference model or a ratio model becomes accessible to important additional criteria: the dimensionality of the obtained Euclidean space and an evaluation of possible artifacts within the obtained configuration.

Since it is possible to represent n points in an $(n - 1)$-dimensional Euclidean space, applications of multidimensional scaling make little substantive contribution when the number of dimensions obtained approaches the number of stimuli. Rather it is desired that the space have few dimensions relative to the number of stimuli, and there exists the possibility that minimum dimen-

sionality may be used as a criterion in comparing different scaling methods on the same stimulus domain.

Helm (1958) applied multidimensional successive intervals to judgments of similarity among hues in a plane of constant lightness, and the data were analyzed using a least squares solution for successive intervals (Diederich, Messick, & Tucker, 1957). Twelve dimensions were extracted for 20 stimuli. The two most significant dimensions reproduced the Munsell configuration quite accurately. However, although the stimuli were arrayed in a markedly nonrandom fashion throughout the remaining dimensions, these higher dimensions seemed to have no direct relevance for the description of perceived relations among the stimuli. When, in accordance with the deduction from the equiprecision notion, an exponential transformation was applied to the distances, the reanalysis yielded precisely two dimensions, and the corresponding scale values reproduced the traditional color circle quite accurately. The simpler structure with fewer dimensions is clearly preferable.

The rationale for the exponential transformation in the multidimensional case is similar to the one given above for unidimensional scales, with a few important supplements:

The obtained scale values, s_{ij}, for similarity between stimulus i and stimulus j cannot be used directly as estimates of distance, since successive interval scale values, being unique only up to a linear transformation, are measured from an arbitrary zero-point. Since the mathematical procedures for this type of multidimensional scaling analysis (Messick, 1956c; Torgerson, 1958) require values invariant up to multiplication by a constant, an additive constant must be determined to translate the relative similarity values, s_{ij}, into distance estimates, x_{ij}:

$$x_{ij} = ks_{ij} + c \qquad [18]$$

Procedures for solving this additive constant problem have been outlined (Messick & Abelson, 1956; Torgerson, 1952). In Helm's first analysis, the additive constant was estimated and x_{ij} was treated directly as a distance; in his second analysis an exponential transformation was applied before the value was considered to represent a distance estimate, d_{ij}:

$$d_{ij} = e^{x_{ij}} = e^{ks_{ij}+c} \qquad [19]$$

Equation 19 may be rewritten as

$$d_{ij} = (e^c)e^{ks_{ij}} = ge^{ks_{ij}} \qquad [20]$$

where g is defined to be the constant e^c.

Since an unknown constant k appears in the exponent of Equation 20, the obtained distances are invariant only up to a power transformation and are hence measured on a logarithmic interval scale. It will be recalled, however, that the dimensional analysis requires distance estimates invariant up to multiplication by a constant. Applications of paired comparisons or successive intervals produce, if the models fit, scale values invariant up to a linear transformation, and solution of the additive constant problem produces the required ratio scale properties. Any nonlinear transformation applied at this point would violate the linear invariance assumption and would throw away the interval metric. However, the transformation suggested above is permissible because the determination in Equation 20 of the unique k to minimize the dimensionality of the obtained Euclidean space places the distance estimates, d_{ij}, on a ratio scale of measurement

invariant up to multiplication by a constant. It should be pointed out that an optimum value for k exists, in that it may be shown that departure in either direction from this value increases either the real dimensionality or the imaginary dimensionality of the space (Helm, 1960).

Helm (1959) carried out a second experiment using a triads ratio scaling procedure (Torgerson, 1958) with 10 of the 20 stimuli used in the previous investigation. The interpoint distances from this study plotted as a straight line through the origin, against the transformed interpoint distances from the successive intervals analysis. This finding, of course, tends to strengthen the previous result.

Further support for the efficacy of the exponential transformation in simplifying complex structures is provided by a reanalysis of some color perception data collected by Mellinger (1956a, 1956b). Twenty-six stimuli varying in brightness, saturation, and hue had been analyzed by Mellinger according to standard procedures for multidimensional successive intervals. The stimuli represented five discrete hues with differing saturations, in the form of spokes around the color wheel, along with some black-white variations for the brightness dimension. His original analysis yielded six dimensions, one for each hue plus a brightness continuum. One large imaginary dimension, apparently produced by contrast effects resulting from differences between chromatic and achromatic stimuli, was also obtained. When the data were reanalyzed with an exponential transformation, only three dimensions appeared, which corresponded quite well to the Munsell configuration. The large imaginary dimension remained essentially unaltered.

DISCUSSION AND SUMMARY

It is suggested that in those scaling situations in which curvilinearities or complex multidimensional structures are obtained by treating the original judgments as difference scores, an exponential transformation may lead to unifying simplifications. Several interpretations may be advanced which are consistent with the data presented in support of this transformation: e.g., difference judgments are made with uniform precision, ratio judgments are elicited by category as well as ratio methods, or the treatment of ratios as differences in the Thurstone procedures leads to curvilinear relations between category and ratio scales and to distortions and artifacts in multidimensional analyses. However, much research is needed to clarify the issue, in particular studies of the influence on scale properties of stimulus characteristics, such as the compelling circular arrays of stimuli used in studying hue, and on the restricting effects of using fixed categories for scaling. Perhaps an insufficient number of categories or the use of an upper anchoring stimulus leads to the underestimation of large differences, and the exponential transformation needs no justification more profound than that it appropriately stretches large distances. Also, if the transformation is thought necessary in a multidimensional analysis because ratio judgments are analyzed as differences, then the treatment of judgments as ratios, as in the ratio triad technique suggested by Torgerson (1958, p. 293), should not require such a transformation (cf. Helm, 1959).

It should be noted that this exponential transformation has been applied in only two multidimensional studies discussed above—both in the area of hue perception. Evidence

suggesting its application was not noted in studies of attitude relationships (Abelson, 1954; Messick, 1956b) or perception of personality (Jackson, Messick, & Solley, 1957). In areas of personality, attitudes, and preference, where positive, neutral, and negative judgments may be made, it is likely that the rationales for the suggested treatment are not relevant and that the transformation will prove of little value. Present evidence suggests that its primary use will be in psychophysics, particularly on prothetic continua.

The primary purpose of the present paper is to present a theoretical rationale for an exponential transformation in category scaling and to suggest the use of this analytical procedure to rectify curvilinearities between category and ratio scales, and to simplify complex, artifactitious multidimensional structures. The ultimate theoretical basis for its use requires much research.

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(Received January 27, 1959)

Psychological Review
1961, Vol. 68, No. 3, 178–189

THE MEASUREMENT OF TRANSITORY ANXIETY[1]

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Probably every psychologist has preferences as to what sorts of facts indicate anxiety. Such preferences are certainly evident in the professional literature. They are also evident in discussions of reported research, for it is not uncommon that a study is disparaged or even dismissed because the empirical measures used are felt to be relatively invalid. It is here, in the reactions of one's peers, that validity assumes its real significance. In fact, we might say that the pragmatic test of a measure's validity is its acceptability to the profession. Only if widespread conventions of usage exist for a concept regarding the manner in which its referent objects or events are manifest in evidence, can professionally valid measures be developed for the concept.

We shall attempt to demonstrate, in the following paper, that such conventions do exist for transitory anxiety. In order to make such a demonstration, we have culled the psychological and psychiatric literature for the types of preferred or criterion evidence and for the objections raised against each of these types. We will not attempt to prove by survey figures which is the best evidence but rather, attempt to reason out the best and so most convincing compromise. Our goal is to promote *agreement* on what constitutes the best evidence of transitory anxiety.

A study of the literature led us to distinguish six types of evidence for transitory anxiety: introspective report, the response to stress, physiological signs, clinical intuition, free molar behaviors, and task performance changes. We shall discuss them in that order, propose a synthetic criterion indicator, and consider the development of more practical indicators by means of predictive validation against the criterion.

Introspective Report

Having once felt anxious, one is rarely in doubt as to whether a certain feeling is anxiety. He may falter if asked whether he is certain that it is not fear, uneasiness, apprehension, dread, anguish, or even terror, but we would expect him to firmly distinguish this class of fear emotions (Hamburg, Sabshin, Board, Grinker, Korchin, Basowitz, Heath, & Persky, 1958, pp. 417–418) from certain other classes of emotions such as anger, grief, erotic desire, elation, or disgust. We have no substantial grounds for believing that the evidence of anxiety's presence should differ in any respect from that of fear but in the possible absence of or error in a report of an object or focus for the emotion. Freud (1943, p. 351) asserts that "there are no means by which the sensations of neurotic anxiety can be distinguished from those of real anxiety (fear)" and Basowitz, Persky, Korchin, and Grinker (1955, p. 3) argue that anxiety and fear must be studied as one. Likewise, we shall not be dis-

[1] This paper represents research supported by a grant, M-516, from the National Institute of Mental Health of the National Institutes of Health, United States Public Health Service, to the University of Michigan Therapy Research Project, E. S. Bordin, Principle Investigator. It has benefitted from the advice and criticism of Nancy Orlinsky and the bibliographic assistance of I. B. Weiner and M. D. Galinsky.

tinguishing among the various fear emotions in our discussion except when we do so explicitly.

The certainty that one who feels afraid or anxious is so, does not seem anywhere contradicted in the psychological or psychiatric literature. Some historians have noted positive grounds for it (Altschule, 1957, Ch. 1; Gardiner, Metcalf, & Beebe-Center, 1937), and some modern authorities have taken a definite stand in its behalf (Grinker, Korchin, Basowitz, Hamburg, Sabshin, Persky, Chevalier, & Board, 1956; Hickham, Cargill, & Golden, 1948; Kubie, 1941; Rapaport, 1942). We suggest that the evidential sufficiency of the private experience of anxiety, for anxiety's being the case, is so widely accepted that it implies a firm convention of usage. Assuming that the reader concurs in this definitional generality, we shall take up several objections to granting introspective report criterion status.

Having asserted that to feel afraid is to be afraid, can we be as certain that one is never afraid at a certain time *unless* he feels afraid at that time, i.e., is the experience of fear necessary as well as sufficient evidence of fear? The answer must be in the negative, for this necessity is challenged by the concept of unconscious anxiety (Freud, 1943, pp. 347–348; Zilboorg, 1933; but Fenichel's conception differs, 1945, pp. 238–239). The popularity of the Psychoanalytic viewpoint gives force to this objection in the light of our reliance upon conventions of usage. We can only counter the objection by noting that any evidence proposed for current unconscious anxiety must be validated also, and its validity depends upon arguments for the alternatives to introspective report.

If we accept reports of anxiety feelings as proof of anxiety, it may appear that we are assuming the referents of these reports to be similar in kind over persons and occasions. This concern with referents derives from the belief that the report only indicates rather than defines anxiety. Once this distinction is made, however, it suggests an objection to the report of anxiety as sufficient evidence of anxiety's presence. A subject may err in his recognition of or inference from certain experiences that he is anxious (Whitehorn, 1939).

The error may derive from the way he has learned to use the word "anxiety" or from a lack of acuteness in self-observation. We can only evaluate the importance and seriousness of this objection if we agree upon certain physical events in or by the organism as defining anxiety and being the referents of its introspective report (as does Arnold in Reymert, 1950, pp. 18–22). This solution would undercut the criterion status of introspective report, as sufficient evidence, but shifts the burden of proof to another class of evidence, the physiological, open to equally serious objections, as we shall show later. Another difficulty of this solution is that such underlying physical events have not been established as the referents of anxiety's introspective report. For example, injections of epinephrine do not reliably produce anxiety feelings (Barcroft, 1955; Lindemann & Finesinger, 1940; Lund, 1939, pp. 54–55), and there is no necessary relationship between visceral sensations and the aspects of anxiety feelings such as a feeling of helplessness or catastrophic expectation (Maslow & Mittelmann, 1941, p. 376), a "scary feeling reported quite separately of sensations" (Williams, 1950), or the appearance of particular fears such as of illness, insecurity, or death (Burchell, 1942; Prados & Ruddick, 1947). Finally,

we need not assume that feelings are necessarily the results of inner physical events at all. They may reflect wholly mental events, whose official recognition is still somewhat unfashionable among psychologists. In fact Nina Bull (1951) reports that subjects distinguish "the feeling of the emotion itself and the various organic sensations" (p. 47), though she also offers evidence of a concomitancy between them (pp. 77–87).

Accepting fear experience as an ultimate (i.e., unanalyzable) proof of fear, we would confront a last problem, that of establishing the *report* of fear as a valid indicator of the fear experience. A subject may be insincere in either his denial or his avowal of feeling afraid. This is, however, a matter of motivation which can be manipulated by instruction and setting. It is not an ineradicable matter of principle, though we cannot *directly* prove a subject's report is sincere. The best we can do is to safeguard against the subject's having any reason for being dishonest, so we must insure that his reports do not test his self-esteem, that he is not reluctant to admit the experimenter to his confidence, that he has a proper set to discriminate and report, etc.

Our argument to this point rests upon conventional usage and the possibility of experimental control in order for us to conclude that the introspective report of anxiety (fear) is *sufficient* evidence that anxiety (fear) is the case. It is not necessary evidence, for many of our peers subscribe to the convention that one may be anxious but not feel anxious. It also may be not ultimate evidence, but a function of some more "basic" evidence. Either of these exceptions endanger the criterion status of introspective report only if there is an adequate substitute for it.

THE RESPONSE TO STRESS

There are certain stimuli that are commonly regarded as sufficient to induce fear. It has long been implicit in such definitions of fear as "arising from the idea of impending evil" (Gardiner et al., 1937, p. 48 [Aristotle], p. 97 [Augustine], p. 158 [Descartes], p. 187 [Hobbes], p. 247 [Condillac]; Hunt, Cole, & Reis, 1958) that such a class of stimuli bears a necessary relation to the fear emotions. This definition of fear was also implicit in the Behaviorist program described by Watson (Murchison, 1928, pp. 40–41). The problem in defining fear as the response to stress is how to establish grounds other than the fear response itself for identifying stressors.

We may fear impending harm to ourselves, our families, and possessions, but only when we are informed of it, for the sort of stressor to which fear is a response is itself a perceptual response. Thus, fear constitutes whatever emotion is caused by the *perception* of impending harm or evil. What the defining evidence of this perception is, we do not know, so we must infer from the objectively impending noxious stimuli and the "orientation" of the subject appropriate for perceiving this threat, to his perception of the threat. If we assume that a proper orientation can be assured, we are left with only the task of defining that set of stimuli which induce a perception of impending harm. These may include physical injury to the person, pain, frustration of his hopes, removal of those aspects of his environment upon which his security depends, loss of self-esteem, or high levels of drive incapable of discharge. All of these are contingent upon attributes of the subject (Lazarus & Baker, 1956, 1957). It is probably impending death, bodily harm, or extreme pain

that we would be most certain of as causing fear, but even these may be impotent as in the case of deeply religious, stoically brave, or deranged persons. Thus, some persons may respond to any given stressor with no emotion or with an emotion other than fear (Funkenstein, King, & Drolette, 1957). Furthermore, what is perceived as merely an inconvenience, nuisance, or breach of manners by most persons may be seen as impending harm by some, so that we cannot describe an exhaustive set of reliable stressors. With the exceptions we noted, however, we can propose some usually sufficient stressors (Basowitz et al., 1955, p. 7), i.e., they will usually produce a perception of impending harm. To safeguard ourselves against the exceptions, we must resort to subjects' perceptual reports regarding the threatening quality of the stimuli with its attendant problem of insuring honest report. The only alternative to this last resort is to define stressors in terms of the emotional response they elicit, which would defeat our purpose here (see Logan, Olmsted, Rosner, Schwartz, & Stevens, 1955, pp. 56–57).

We have argued, then, that defining anxiety as response to stress rests like introspective report upon convention, namely, the irresistible fearsomeness of certain stimuli, but a less determinate convention. The stimuli productive of fear may be somewhat idiosyncratic for any subject and even the most potent stimuli can conceivably fail to produce fear. Thus, no given stressor is a necessary or sufficient cause of fear, though some would generally be expected to cause it in most persons.

A Set of Physical Signs

Speculation about the physiological basis of the emotions has been a preoccupation of theorists since at least 400 B. C. (Gardiner et al., 1937, p. 2) and physical signs have been noted for as long a time. Thus, the necessity of physiological involvement in emotion has long been a popular doctrine, but the differential assignment of the rather large number of possible physical signs, so as to provide a sufficient set of evidence for the presence of anxiety, has not yet been accomplished. If it is to be accomplished independently of introspective report which was used as the criterion in the early experimental studies (Cannon, 1915, pp. viii, 1–4; Woodworth, 1938, pp. 257–297), and stressors, used, e.g., in Darwin (1915, pp. 289–308) and many recent studies, a specific physiological response entity must be distinguished. The variables out of which this response entity (probably a multivariate response pattern) may be developed have been studied by a great variety of investigators (Krause, Galinsky, & Weiner, 1961).

Any particular physiological variable is usually justified as a measure of anxiety by the argument that it indicates autonomic or, better, adrenosympathetic activation (perhaps reticular activation will soon displace this —Malmo, 1959). The convention upon which this argument rests is that one cannot be anxious or afraid without this sort of physiological involvement. The point is often made in a more extreme form equating the emotion with a "certain" physical state (e.g., see Wenger's or Arnold's articles in Reymert, 1950). The most immediate problem facing the argument is that of defining adrenosympathetic (a-s) activation. If we consider *any* effect produced by epinephrine or sympathetic division discharge, and not explicable in the given situation on grounds other than emotional arousal (like room temperature changes, operant behavior, fatigue, etc.)

as sufficient evidence of a-s activation, then a positive indication by any particular relevant variable would be necessary evidence of anxiety. This result leads us on to two further problems. If a-s activation indicates the fear emotions, does it not also indicate other emotions or quasi-emotional states as well? It is certainly as much of a convention that anger involves a-s activation as that fear does. Therefore, indication of a-s activation is, indeed, not sufficient evidence that a fear emotion is present. Further evidence is needed to rule out at least the possibility of anger. To specify what constitutes this evidence, we must go outside of a purely physiological framework for criterion measures of fear and anger. Physiological differentiations between these two sorts of emotions have been offered (Ax, 1953; Ribot, 1914, pp. 207–229; Schachter, 1957), but always via recourse to introspective report or knowledge of the stressor. If a positive indication by any relevant measure is sufficient evidence of a-s activation, as we proposed, then to insure the absence of a-s activation, all of the relevant measures must be canvassed, *except* if they should vary concomitantly. It has become increasingly apparent that they do not vary concomitantly and that the pattern of response may differ between persons, though individuals are somewhat consistent in their response patterns (Ax, 1953 [the mean intercorrelation over seven variables under the fear condition was −.09]; Cameron, 1944, 1948; Finesinger, 1944; Lacey, 1950, 1959; Lacey, Bateman, & Van Lehn, 1953; Lacey & Van Lehn, 1952; Mandler & Kremen, 1958; Shepard, 1906 p. 557; Stopol, 1954; Wittkower, 1935, pp. 570–71—for a general discussion see Bindra, 1959, pp. 219–228). Therefore, an experimenter must determine which a-s variables are most reliable for each subject, in order to safeguard himself against false negative indications of a-s activation.

Although evidence of a-s activation may be, by convention, necessary evidence of fear, it does not constitute sufficient evidence. Therefore, it cannot be an independent criterion measure of anxiety without further validation. This does not close the matter of physical signs, for we must consider the more molar ones used by experienced clinicians and other observers of human behavior to recognize transitory anxiety states.

CLINICAL INTUITION AND MOLAR SIGNS

Just as the awareness of one's own feeling of fear may be an ultimate fact which cannot be analyzed into simpler, more objective constituents, so may be the judgments of a trained and experienced clinician as to the presence of anxiety in a patient. The striking disanalogy between the two cases lies in the privileged access of introspection to feelings as against the absence of any such privilege for the clinician. Insofar as we know, he must exercise his intuition upon public events. Thus, one may question his judgments by requesting that he specify the events upon which the judgments rest. This skeptical position rejects the ultimacy, though not the usefulness, of clinical intuition and prefers to rest upon physical signs. It is implicit in studies of clinical rating of anxiety (Buss, Wiener, Durkee, & Baer, 1955; Cattell & Scheier, 1958; De Michele, 1954; Hamburg et al., 1958). Although some clinicians may be exquisitely sensitive to anxiety in others, what we have said above suggests that the clinician must engage in an inferential process, probably covert, rather than

directly perceiving anxiety in the same sense as he perceives colors. We are in effect asserting that though one may directly experience his own anxiety, he can only perceive indications of another's anxiety.

Beside the physiological variables already discussed, there is another sort of physical sign which may be called "molar." By this we mean to include body posture, gesturing, speech characteristics, and facial expression (for a more detailed list see Ruesch, 1953, p. 159). These presumably constitute the evidence used by clinicians. The most serious argument for granting certain of these signs criterion status in the definition of the fear emotions was offered by Tolman (1958):

> The response of . . . clutching, blinking, puckering of lips, crying, running, and hiding seem all . . . to be of a nature to protect the organisms from exciting stimuli of one sort or another. . . . They are all protection responses. And it is in this character that they alone identify the instance as one of fear (p. 27).

He shifts the burden from an intuitive recognition of fear to an intuitive recognition of protection responses. The major objection to this is the equation of fear and defense, for one may both be afraid and not defend himself, as well as defend himself without fear (though, perhaps, this is not so in the case of infants, from which Tolman took his examples). The paradigm circumstances for Tolman's argument involve the observation of subjects who are aware of a serious, imminent, and present danger. Most of us could recognize the molar signs of fear in such subjects' behavior and, if we were certain they had no reason to simulate fear, confidently assert that they were afraid. Without knowledge of the stressor, however, the judgment would be more difficult, for the molar signs themselves are often ambiguous (Bruner & Tagiuri, 1954, pp. 636–637), but if we observed an extended sequence of each subjects' behavior, we could undoubtedly agree that certain sequences connoted fear, i.e., made us convinced of the subjects' fear (Hebb, 1946). These behavior sequences constitute criterion evidence by convention. Their most characteristic feature is probably the posture of flight or paralysis (see Bull, 1951, pp. 100–104; Hanfman, 1950, p. 62; Spiegel, 1955, p. 39), bolstered by facial expression and other detail as evidence that it was not calculated flight connoting mere discretion. If the danger to the subject is not present, imminent, and serious, his behavior will lack the distinctive "focus" apparent in fearful flight or paralysis.[2] Thus, the experimental study of the fear emotions by means of these criteria is limited to the extent that we can control or have access to such severe stressors. However, since other responses than flight or paralysis may result from even these stressors, our two molar behavior sequences constitute sufficient, but not necessary, evidence of fear.

The various molar signs taken individually are neither necessary nor sufficient evidence of fear, for neither claim is generally made in the studies employing them: gesture (Sainsbury, 1955), posture (Bull & Gidro-Frank, 1950, Conklin & Dimmick, 1925), speech (Dibner, 1956; Mahl, 1956), and facial expression (Bruner & Tagiuri, 1954, pp. 634–638). These indicators are not only susceptible to voluntary inhibition, but individuals

[2] It is this lack of focus that distinguishes (neurotic) anxiety from the more recognizable forms of fear, though only when anxiety does not gravitate to some more or less "innocent" object (Freud, 1943, p. 351; 1953, p. 314).

differ markedly in their relative use. We must conclude that these molar signs taken individually do no qualify as criterion evidence of anxiety.

Before we attempt to compound the various criteria, a brief remark about the distinctions we have made between various sorts of physical signs is in order. We are aware that the line between physiological and molar signs is not clear, but the ambiguity is not prejudicial to our arguments. On the other hand, we have excluded from consideration a large collection of measures which might be called "effects on task performance" and are also not always easily distinguished from the molar or physiological signs. Our intention was to separate the evidence of fear from the evidence of its effects, but the separation is arbitrary, for we lack the strength and precision of definitional convention to restrict fear to a certain collection of evidence. Thus, a comprehensive treatment of our topic ought to include the measures of task performance effects. These are already surveyed in Horvath (1960) and Lazarus, Deese, and Osler (1952). The status of these measures as evidence of fear is like that of the individual molar signs, for though they may testify to the presence of some emotion, they are not sufficient evidence that it is specifically fear. Furthermore, one may certainly be afraid, at least moderately, without it showing in most task performances, so even as supporting evidence they are only with conventional weight for the case of intense emotion.

SYNTHESIS

Each criterion measure we considered was open to certain reasonable objections or restrictions. Our discussion of possible criterion indicators was in essence an appeal to usage with regard to affirmative proposals as well as objections and restrictions. We have attempted to infer the conventions of usage from the professional literature on anxiety, fear, and emotion, but the practical test of the inference lies as much in the reader's intuitive assent to our definitional estimations as in his ability to replicate our efforts. In the absence of such assent to *some* definitional formulations concerning anxiety, empirical research on the concept tends to be esoteric, for questions of validity cannot be resolved. This assent is the manifestation of conventions of usage. Where it is absent, such conventions are absent, and they are as essential to the cumulation of research efforts as are detailed reports of experimental design and methods, the derivation of conclusions by conventional inference procedures, and the development of rigorously stated theories.

The restrictions and objections to the various criterion indicators can be avoided by combining indicators, drawing upon the evidential strength of one to supplement the weakness of another. The strongest convention was, we believe, that of the sufficiency of feeling afraid as evidence of fear. The objection that this feeling is merely a corrigible perception of internal events is moot, for we know as yet no reliable pattern of internal events to substitute for it, though the work of Bull (1951) is suggestive. The expression of the feeling through introspective report introduces problems of accuracy and honesty, which must be met by providing subjects with motives and sets conducive to careful, honest reporting, as well as by recourse to other supplementary definitional conventions.

Although we would not go farther than to assert that certain stressors are likely to induce fear, we can study subjects in a state of fear with least

chance of false positive instances if we use these stressors. Likewise, the benign effect of certain other stimuli or circumstances is so likely that we can avoid most false negative instances by exposing subjects to these stimuli for our study of the absence of a state of fear.

The necessity of physiological involvement to a state of fear suggests measuring a-s activation as a check upon the honesty of introspective reporting. It cannot as yet help us to assure discrimination of the fear emotions from others like anger, however. For this assurance we must rest upon our preparation of the subject and choice of stressor.

Finally, the evidential sufficiency of a flight or paralysis sequence by the subject in response to a probable stressor is just about as secure a convention as that concerning the fear affect. This partial criterion might be used to supplement the latter and vice versa, but the circumstances favoring flight or paralysis are more restricted than those appropriate to introspective report, so the two sufficient indicators are not generally interchangeable.

The only strong definitional assertions we have made are that a feeling of fear and a flight or paralysis response sequence are sufficient, while physiological involvement (a-s activation) is necessary evidence of a state of fear. The conjunction of these assertions logically implies that one who feels fear or impulsively flees or is functionally paralyzed must suffer a-s activation at the same time. Since neither sufficient criterion is also necessary to a state of fear, they do not logically imply each other. When we find a subject who feels afraid or who impulsively flees or is functionally paralyzed in response to a probable stressor, and shows a-s activation as well, we cannot be more certain that he is in a state of fear. The more global and far-reaching the apparent consequences of these facts in his appearance and behavior, however, the greater our ease in recognizing the emotion. Thus, extreme fear is more recognizable than moderate fear or anxiety.

VALIDATING PRACTICAL INDICATORS

The forms of evidence we have asserted to be the best evidence of anxiety are very limiting for empirical research. If we wish to collect evidence upon anxiety feelings, we are restricted to subjects accustomed to introspecting and reporting their feelings and to situations conducive to accurate and honest reporting. The physiological recording apparatus necessary to measure a-s activity is extremely expensive and restrictive of the subjects' movement. The collection of either of these forms of evidence to some extent precludes spontaneity or naturalness of subject behavior. The occurrence of flight or paralysis interferes with careful introspection and physiological recording, as well as severely restricting the range of stressors suitable for use, the range of emotional intensities evoked (to the higher levels), and aggravating the ethical and recruiting problems involved.

The fact that the most valid evidence may not be the most sensitive, economical, or feasible to collect, leads us to develop secondary indicators which *are* adequate in these respects. The considerations of practicality and validity are independent of one another but both are of crucial importance. We shall not attempt to treat the various practicality issues here, but we shall sketch the procedure for validating promising secondary indicators. Briefly, the concomitancy of these secondary indicators with the

(compound) criterion indicators, under the conditions appropriate to these latter, i.e., in a validation experiment, testifies to the secondary indicators' validity.[3] We shall close our discussion with a description of the essential features of such a validation experiment and some problems to be encountered in carrying it out.

Each subject is exposed to stimuli of two sorts, one of presumably stressful nature for him and the other presumably innocuous. His dependent-variable responses will be studied only for those exposures which induce both an "honest" affective report and a confirming a-s reaction. Therefore, the conjunction of stressor, reported fear, and a-s activation defines the fear condition (a combination of treatments and classificatory variables), while the conjunction of inocuous stimulus, reported absence of fear, and significantly less or no a-s activation defines the no-fear condition. This follows logically from our discussion of criterion evidence. Exposures which do not produce just these conjunctions will not produce useable data for this research. Precautions can be taken to minimize such waste, however.

First, each subject ought to be pretested to determine his initial anxiety (or a-s activation) level. Most of the relevant studies indicate that some initial anxiety results in a greater degree of physiological response to stress (Basowitz, Korchin, & Grinker, 1954; Eichhorn & Tracktir, 1955; Jacobson, 1929, p. 125; Jost, Ruilman, Hill, & Gulo, 1952a, 1952b; Malmo & Shagass, 1949; Piercy, Elithorn, Pratt, & Crosskey, 1955; Seward & Seward, 1935; White & Gildea, 1937), but others indicate that very high initial levels allow for a smaller response (Glickstein, Chevalier, Korchin, Basowitz, Sabshin, Hamburg, & Grinker, 1957; Lacey, 1956–57; Persky, Grinker, Hamburg, Sabshin, Korchin, Basowitz, & Chevalier, 1956; Persky, Korchin, Basowitz, Board, Sabshin, Hamburg, & Grinker, 1959). For some suggestive analyses see Fischer and Agnew (1955), Malmo (1959, p. 379), and Wilder (1950). The danger is that initially anxious subjects may either respond to all stimuli or none, which would make one or the other of our experimental conditions difficult to achieve.

Second, chronically anxious subjects may be responding to the environment with fear due to their broader generalization (Lacey, Smith, & Green, 1955; Rosenbaum, 1950) and easier conditionability (Bitterman & Holtzman, 1952; Spence, 1956, pp. 179–189) of fear responses. This would tend to render subjects unusable for fear, nonfear comparisons, because of the rarity of achievement of the nonfear condition.

Third, stressors to which the subject is accustomed through outside experience or repeated experimental exposure may be less effective (especially for autonomic responses—Malmo, Shagass, & Heslam, 1957; Martin, 1956; Rowland, 1936). Getting information on this matter as well as on the likely stressfulness or innocuousness of the experimental stim-

[3] An especially desirable extension of anxiety measurement, beyond those instances in which the criterion might in principle be appealed to, concerns the matter of unconscious anxiety. The presence of a likely stressor, a-s activation, and the honest denial of feeling anxious constitute a likely background for the inference of anxiety on the basis of positive indication by a well-validated concomitant. The inference would be further confirmed by the subject's subsequent feeling of fear to the same stressor, or awareness of the feeling upon subsequent recall. Thus, we take the position that with our present definitional conventions, the question of unconscious anxiety is an empirical matter.

ulus repertoire could insure less wasted exposures.

Beside the preceding economic considerations, we must emphasize the desirability of separate analyses for each subject's data. This is necessitated by the likelihood that subjects will respond idiosyncratically to the same stressor (the supporting literature was cited above), but it may be possible to predict a subject's mode of response (e.g., Fisher & Cleveland, 1957). Furthermore, a variety of stimuli should be used for each subject to allow for specific stimulus effects to be washed out (Davis, 1957).

It is our hope that the validation of anxiety indicators against our proposed criterion, or some other better substantiated one, promotes a greater convergence and cumulation of research on this important variable.

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(Received September 14, 1959)

Psychological Review
1961, Vol. 68, No. 3, 190-204

VERBAL HABIT-FAMILIES, CONCEPTS, AND THE OPERANT CONDITIONING OF WORD CLASSES [1]

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In several papers Hull (1934a, 1934b) has described the concept of the habit-family which he felt would

> prove to have an extremely wide application as an explanatory principle in many subtle and otherwise inexplicable forms of behavior at present usually designated indiscriminately as intelligence (1943b, p. 147).

The concept has already been applied by Hull and others and this paper falls into the category of additionally, or more specifically, making applications to certain complex human behaviors. Before discussing the applications, it would seem useful to give a short summary of the concept of habit-families.

Figure 1 schematizes a habit-family, in a somewhat simplified way. In the figure a part of a response which originally was elicited by the final stimuli in a sequence becomes elicitable by earlier stimuli in the sequence. This partial response is called the fractional anticipatory goal response, rg. Since rg may be elicited by the stimulus at the beginning of a sequence this rg may precede other, instrumental, responses elicited by the stimulus. When this occurs the rg and the stimuli it produces are contigous with the instrumental responses and these stimuli will come to elicit the instrumental responses R_1, R_2, and R_3 in a divergent mechanism. The stimuli produced by these three responses will be associated with the goal response, R_g, and tend to elicit the goal response (including the portion which becomes the rg) in a convergent mechanism. Thus, a simple habit-family could be summarized as a stimulus which has tendencies to elicit an anticipatory goal response which in turn has tendencies to elicit a hierarchy of responses, each of which tends to elicit a common final response, part of

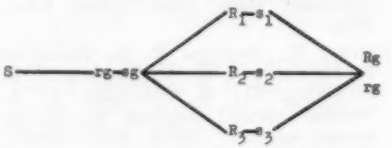


FIG. 1. A habit-family. (Direct associations between S and the instrumental responses are not depicted in this and the next figure.)

which is the anticipatory response. This summary ignores the direct associations between the original stimulus and the instrumental responses.

Mediated generalization would occur from one instrumental response to the others.

THE CONCEPT OF THE HABIT-FAMILY IN LANGUAGE BEHAVIOR

Cofer and associates (Cofer, 1951, 1957; Judson & Cofer, 1956; Judson, Cofer, & Gelfand, 1956) in a series of papers on reasoning use the term habit-families in respect to language behavior in describing the course of reasoning, but the term is never given elaboration,

[1] This article arose from research sponsored by the Office of Naval Research under Contract Nonr-2794(02) and the National Institute of Mental Health under Contract M-2381. A longer version of this article was issued August 1959, under the same title, as *ONR Technical Report* Number 10. The author wishes to thank Charles E. Osgood and Carolyn K. Staats for critically reading the manuscript and for their ensuing suggestions.

and sometimes seems to be of varying nature. Thus, Cofer seems to conceive of habit-families in thinking both on the basis of semantic characteristics of the words and also in terms of the direct associations between the words. In this sense, clusters of words which are related to each other in any way could be termed habit-families.[2]

Osgood (1953, 1957a, 1957b; Osgood, Suci, & Tannenbaum, 1957) has applied Hullian mechanisms to language in frequently using the concepts of the convergent and divergent mechanisms. He has stated (1957a) that different environmental stimuli could become associated with the same mediating response, a convergent mechanism, and that this mediating response (or its stimuli) could have tendencies to elicit various instrumental responses, a divergent mechanism. In addition, a sensory signal may come to elicit more than one mediating response in a divergent hierarchy. As an example, he cites homophones like *"case, bear,* and *right"* (1957a, p. 98). since they each elicit more than one meaning. Thus, much of the development of the habit-family in its application to language has been made by Osgood. However, his model differs from the fuller application of Hull's concept to be developed herein in certain important respects, and the detailed description of the verbal habit-family and its development and function, must still be made. Greater clarity and detail of the verbal habit-family model should have significant heuristic value.

The principles of language learning which are relevant to verbal habit-families will first be summarized and then organized to illustrate the concept. Then the concept will be applied to concepts, concept formation, and the operant conditioning of word classes.

[2] Direct associations between words will not be discussed herein. However, it is realized that these associations have an effect upon the organizational aspects of language.

VERBAL HABIT-FAMILIES

Classical Conditioning of Word Meaning

Cofer and Foley (1942), Mowrer (1954), Osgood (1953), and Staats and Staats (1959a, 1959b) have discussed word meaning in terms of Hullian concepts as an implicit, mediating, response. A distillation of these views for the present purposes might state, as the first principle in which this paper will be interested, that when a word is contiguously presented with a stimulus object some of the unconditioned responses elicited by the object will be conditioned to the word. These responses when stably conditioned become the meaning of the word. First-order conditioning of meaning has been demonstrated by Staats, Staats, and Crawford (1958). Additional support for the contention that the concept of meaning may be treated as a conditioned response is given by a series of studies by Staats and Staats and associates (Staats, 1959; Staats & Staats, 1957,1958, 1959a, 1959b; Staats, Staats, & Biggs, 1958; Staats, Staats & Heard, 1959, 1960). A higher-order conditioning paradigm was involved in these studies since conditioning was accomplished by pairing a word which already elicited a meaning response with the verbal stimulus which was conditioned to elicit that meaning. Both connotative and denotative meaning responses were conditioned in this manner.

Osgood's concept of a representational mediating response which may form the meaning of a word is an elaboration of Hull's (1930) rg, or "pure stimulus act." According to Osgood (1953) this concept includes conditioned autonomic responses and implicit motor responses. The present

account elaborates this conception to include conditioned sensory responses. That is, it is suggested that some stimulus objects elicit sensory responses in organisms, parts of which may be conditioned. Thus, parts of the "seeing," "hearing," or "feeling" responses, for example, elicited by the appropriate stimulus on an unconditioned basis may be conditioned to other neutral stimuli, including verbal stimuli.

Studies (e.g., Leuba, 1940; Lipton & Blanton, 1957; Philips, 1958) [3] indicate that sensory responses may be conditioned. Skinner discusses in detail (1953, pp. 266–270) how sensory responses can come to be elicited by formerly neutral stimuli on the basis of classical conditioning.

> A man may see or hear "stimuli which are not present" on the pattern of the conditioned reflexes: he may see X, not only when X is present, but when any stimulus which has frequently accompanied X is present. The dinner bell not only makes our mouth water, it makes us see food (p. 266). (Quoted by permission of the Macmillan Company.)

More recently, Mowrer (1960) has also discussed the conditioning of sensory responses in describing sensory preconditioning studies.

> we may confidently assume that the light . . . produces a light *sensation* . . . which is conditionable in the form of a light *image*. Such a reaction to be sure, is central or "cognitive," rather than overt, behavioral; but emotions also are covert, nonbehavioral, yet we have not refused . . . to admit them to our theoretical system (p. 282).

Following the conception that sensory responses may be conditioned, the pairing of the auditory presentation of the word BALL and the visual presentation of the object ball, as an example, would be expected to condition the child to respond to the auditory verbal stimulus with part of the visual responses elicited by the ball itself. The word stimulus would now be meaningful. Additionally, the child's own speech response BALL can be considered to be equivalent to the words as a stimulus since it produces the same type of sound. Thus, after the conditioning experience, both the presentation of the word by some other person or the saying of it himself would elicit the meaning response.

[3] Mowrer (1960) and Staats, Staats, and Heard (1960) have concluded that sensory preconditioning depends upon the conditioning of sensory responses. This interpretation allows a rapprochement between S-S and S-R theoretical orientations.

Operant Conditioning of Word Responses

In addition to the learning of word meanings, the individual learns through operant conditioning to emit verbal responses. Skinner (1957) has developed at length the principles by which these responses are acquired, and a number of experimental studies as summarized elsewhere (Krasner, 1958; Salzinger, 1959) have shown that verbal responses may be strengthened through the action of reinforcement. It might be expected that precise speech responses, for example, are shaped up by selective reinforcement in the manner described as successive approximation. Thus, at an early age, any sound emitted by the child that sounds like an English word is reinforced. Later only more and more precise utterings receive reinforcement and so on.

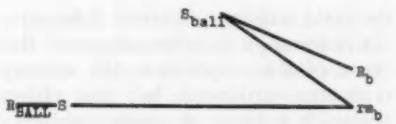
Skinner also described the principles by which environmental stimuli come to control certain verbal responses of the individual, or, in common sense terms, the way the child learns to name objects. The principle involved is that of operant discrimination. If, in the presence of a stimulus object, the child is reinforced when he utters a particular name or word, the stimulus object when presented will tend to elicit that verbal response from the in-

dividual.[4] Emitting a labeling response under the control of the appropriate environmental stimulus is called "tacting" by Skinner.

Meaning as an Anticipatory Response

According to the conventions of this paper the conditionable sensory component elicited by the object ball may be conditioned to the stimulus word BALL. In addition, if the child is reinforced after saying BALL while responding to the object, as occurs in the operant conditioning of verbal behavior, an association is established between the stimuli produced by the sensory responses to the ball and the speech response. The formation of both of these associations is depicted in Figure 2. Thus, through classical conditioning, the conditionable sensory responses elicited by the ball, rm_b–s, are conditioned to the word response BALL, R_{BALL}–S. In addition, when the child is reinforced for saying BALL while looking at the ball, the verbal response is conditioned to the same sensory responses, rm_b–s. Thus, the word response R_{BALL}–S tends to elicit rm_b–s, but rm_b–s also tends to elicit the word response.

When the meaning response (conditionable sensory response) which has been conditioned to a word response has also become anticipatory to the word response, the meaning response is analogous to Hull's rg. That is, an rg is conditioned to the stimuli produced by preceding instrumental responses, but also comes to elicit those instrumental responses in the process of short circuiting.



2a. The conditioning of a meaning response to the stimuli produced by a word response. (When the mother says BALL, R_{BALL}–S, and presents the ball S_{ball}, part of the sensory responses elicited by the object, rm_b, are conditioned to the auditory stimuli. Part of the responses elicited by the ball, R_b, are not conditionable.)

2b. The conditioning of a word response to the stimuli produced by the conditionable sensory response components. (When the child is reinforced for saying BALL in the presence of the ball, the speech response, R_{BALL}, comes under the control of the stimuli produced by the conditionable sensory response components, rm_b—sm.)

FIG. 2. The conditioning of meaning and word responses.

Divergent Mechanism

It will be remembered that Hull's habit-family consisted of a stimulus which would elicit an anticipatory response which had tendencies to elicit a divergent hierarchy of responses, each of these responses having tendencies to elicit the same goal response, part of which was the anticipatory goal response. To continue with the example, not only will the naming response BALL be reinforced in the presence of the stimulus object ball (and similar stimulus objects), but in addition a number of other word responses will receive the same treatment, e.g., ROUND, CIRCULAR, SPHERICAL. That is, in the presence of the ball,

[4] It is cumbersome to repeatedly use the terminology concerned with operant conditioning, i.e., a discriminative stimulus gains control over the emission of the verbal response reinforced in its presence. In the present paper it may be simply stated that a stimulus tends to elicit a response whether the principle involved is classical or operant conditioning.

the child will be reinforced if he says, "It is round, it is spherical, etc." Because of this experience, the sensory responses produced by the object (actually a class of similar objects) will come to tend to elicit each of these speech responses in a divergent hierarchy of responses. Thus, the sensory responses elicited by an object, including the portion which is conditionable, come to control more than one tacting response.

Convergent Mechanism

In addition, each of these stimulus words (or word responses, through the stimuli they produce) will have been conditioned to elicit the same end response component, their common meaning. This forms a convergent hierarchy. Each of the set of words or word responses of the individual comes to elicit the common meaning in the manner previously described as the first-order conditioning of meaning, e.g., through being paired with the object ball when someone says, "This is round," in the presence of the object.

The Mechanisms as a Verbal Habit-Family

These divergent and convergent hierarchies (including both meaning and word responses) actually compose a habit-family, united by the common meaning response component. That is, an "anticipatory" response (rm_r–s in Figure 3) has been formed which when elicited by a stimulus will tend to elicit a class of responses (R_{ROUND}, $R_{CIRCULAR}$, $R_{SPHERICAL}$, R_{BALL}, and $R_{ORANGES}$) all of which (through the stimuli they produce) culminate in the elicitation of a common response. In terms of the language responses, a verbal habit-family exists when an anticipatory meaning response component elicited by a stimulus has tendencies to elicit a class of word responses and each of these word responses has tendencies to elicit the same common meaning response component. The verbal habit-family in the example is depicted in Figure 3..

Many different stimuli could tend to arouse the verbal habit-family through eliciting the anticipatory meaning response, e.g., verbal and nonverbal stimuli conditioned to elicit the anticipatory meaning response, the objects involved in the original conditioning since they elicit the conditionable sensory components, and objects which elicit similar sensory components.

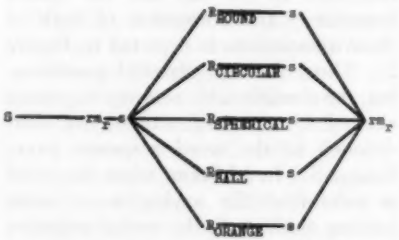


FIG. 3. The verbal habit-family for "roundness."

Thus, there could well be a hierarchy of stimulus situations with varying strengths for eliciting the anticipatory meaning response and, thus the verbal habit-family. This would be analagous to Maltzman's (1955) compound habit-family. Any variable which strengthened the association of the anticipatory meaning response to a stimulus (where this is appropriate) would strengthen each of the individual word responses in the verbal habit-family, mediated by the anticipatory meaning.

To further complicate the situation, since meaning is composed of independent response components, a word response could be in more than one verbal habit-family. And a stimulus situation (or stimulus word) could

elicit more than one unconditioned or conditioned anticipatory meaning component, and thus have tendencies to elicit more than one verbal habit-family.

CONCEPTS

Hull (1920) originally posited that concepts are developed by abstracting the common stimulus elements in a series of stimulus objects. In his experimental demonstration of this approach he used as stimulus objects groups of Chinese characters. For each group there was a common component imbedded in each individual character. The subjects' task was to respond with a particular nonsense syllable to a group of characters. It was found that the subjects improved in anticipating correctly the syllable name of a new character, after having experience with other characters containing the same element. That is, they were able to "abstract" the common elements.

Osgood (1953), however, feels that consideration of concept formation as the abstraction of identical stimulus elements would not distinguish the process from all learning, making the term useless. He states that even most lower animals could learn to do what Hull showed in his experiment.

> Fields . . . showed that rats could learn to jump toward a triangular form. . . . Yet, should we conclude that the rat can understand the *abstract* concept of triangularity? Would the rat respond positively to three dots in a triangular arrangement versus four dots arranged in a square? Or react positively to three people, three places on a map, a three-cornered block, as "triangles." . . . It would seem that the only *essential* condition for concept formation is the learning of a common mediating response (which is the meaning of the concept) for a group of objects or situations, identical elements and common perceptual relations merely facilitating the establishment of such mediators (pp. 667–668).

Osgood, while rejecting the notion that concepts are based upon identical stimulus elements, does not adequately specify how the objects come to elicit a common response or what the common response is. If the three dots, for example, do not elicit a response like that elicited by a triangle on an unconditioned basis, how do they come to do so? How is the power of abstraction gained? The processes involved must be specified to a much greater extent before concepts can be accounted for in S-R terms.

Kendler and associates (Kendler & D'Amato, 1955; Kendler & Karasik, 1958; Kendler & Mayzner, 1956; Kendler & Vineberg, 1954) have also considered concept formation to be the acquisition of a common implicit response to different stimuli. In addition, Kendler and Karasik (1958) have extended this to verbal concept formation which they assume occurs "when *S* learns to respond to a set of different words with the same implicit response" (p. 278). The conceptualization that words which elicit a common implicit response are involved in a verbal concept begins to focus on the verbal aspect of concepts in the "two-stage" S-R framework. However, further specification as to the processes of the development and function of verbal concepts is necessary, since the common meaning response to the words used by Kendler and Karasik had been acquired by the subjects prior to the experiment.

The following discussion will attempt the necessary elaborations. A concept may be regarded as a verbal habit-family formed usually on the basis of a class of stimulus objects having identical elements. Take, for example, the "animal" concept. The individual words in the concept will gain their meaning through classical conditioning where the word is paired with the ap-

propriate stimulus object (actually a number of stimulus objects having closely common characteristics). DOG is paired with dogs, COW with cows, and so on, and the conditionable sensory components elicited by the stimulus object are conditioned to the word involved.

Now each of the stimulus objects in the class has certain identical elements (e.g., legs, head, spontaneous movement, furry, etc.) and the objects in the class will thus elicit sensory response components which also have identical elements. Consequently, part of the meaning response component conditioned to the word DOG will be identical to those which, in the same manner, are conditioned to the words COW, HORSE, and PIG, etc. This common response could be called the animal meaning response component, rm_a in Figure 4.

In addition, however, each stimulus object elicits conditionable sensory response components which the other objects in the class do not. Since these conditionable sensory responses are characteristically elicited only by the specific animal, they are only conditioned to the specific animal word with which the object contiguously occurs. Thus, each of the animal words comes to elicit an animal meaning response component shared by the others, and also a specific meaning response component which none of the others elicit, rm_d, rm_c, rm_h, or rm_p in the figure.

Now, each of the stimulus objects will also occur in the presence of the word ANIMAL. For example, in the presence of each animal the child will be told, "That is an animal." In this process the common conditionable sensory response component elicited by all the animals, the animal component, will be strongly conditioned to the word ANIMAL, and each of the specific conditionable response components elicited by only one of the animals will be weakly conditioned to the word ANIMAL.

The meaning responses which are conditioned to the words will also become anticipatory to the words through the tacting process which has been

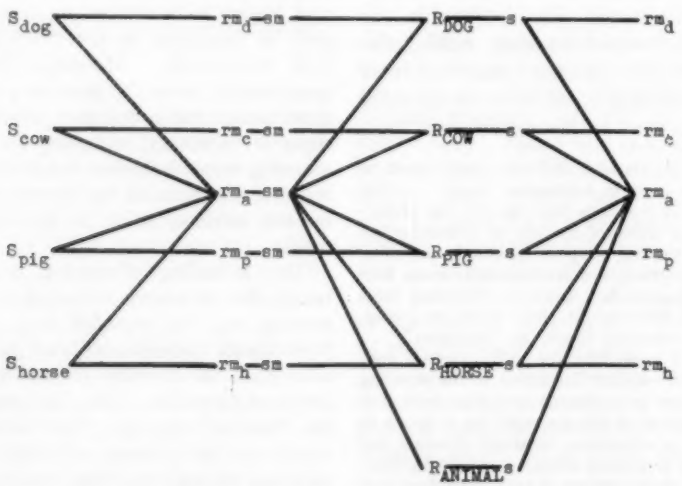


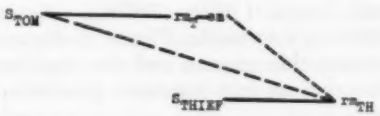
FIG. 4. The "animal" concept.

described. Thus, the animal meaning response and the animal word responses form a verbal habit-family as Figure 4 indicates.

The associations depicted in Figure 4 indicate why, in the presence of a particular stimulus object in the class, the individual is likely to specifically "label" the stimulus object rather than to say ANIMAL, or one of the other words. For example, the stimulus object cow would elicit the sensory response component, rm_a, which in turn would equally tend to elicit all of the animal word responses, including COW and ANIMAL. Thus, all of the words would have an equal probability of occurring simply on the basis of this association. The stimulus object cow would elicit in addition, however, the characteristic sensory response, rm_c, which in turn would strongly tend to elicit the word response COW and, also, though not so strongly, the word response ANIMAL—but none of the other word responses. Thus in the presence of the stimulus object cow there are two strong associations for the elicitation of the word response COW, one strong and one weak association for the word response ANIMAL, and only one strong association for the elicitation of any other word in the concept class.

But this does not yet account for abstraction, i.e., how the individual comes to respond in the same manner to a new object which does not have identical stimulus elements with the other objects in the class and so does not elicit the common response, e.g., how one responds to three dots as a triangle. It would seem that abstraction first comes from the verbal habit-family hierarchy. To realize the explanatory value of the verbal habit-family, the principles of communication which have been presented by Mowrer (1954) must first be summarized.

Using a conception of meaning such



5a. As a result of the sentence, the meaning of THIEF, rm_{TH}, is conditioned to the word TOM and also to the stimuli produced by the meaning response elicited by TOM, rm_T—sm.

5b. The word TOM, S_{TOM}, now elicits the meaning of the word THIEF, mediated by rm_T—sm.

5c. The same is true for the person himself, S_{TOM}, since he also elicits rm_T—sm on an unconditioned basis.

FIG. 5. Mowrer's conception of communication.

as had been described herein, i.e., an implicit mediating response, Mowrer has suggested that a sentence is a conditioning device and that communication takes place when the meaning response which has been elicited by the predicate is conditioned to the subject of the sentence.[5] In addition, since the subject of the sentence will also elicit a meaning response which has stimulus properties, the meaning response elicited by the predicate will be conditioned to the stimuli produced by the meaning response elicited by the subject of the sentence. That a meaning response may be conditioned to the stimuli produced by another meaning response has been shown by Staats

[5] The view of communication taken by Osgood, Suci, and Tannenbaum (1957) would consider the above interpretation of communication to be oversimplified. It is beyond the scope of the present paper to discuss the process of communication in detail. However, the final principles accepted concerning communication can be incorporated into the present model of concept formation.

and Staats (1959a, 1959b). Using Mowrer's example, Figure 5 demonstrates this process and the resulting associations and mediated generalization.

The function of the verbal habit-family in making abstraction possible may now be described, i.e., how stimulus objects get into the concept class when they do not have identical stimulus elements and so do not elicit responses common to the objects which are in the class. Returning to the animal concept example, the individual having learned a meaning response to the word ANIMAL will be conditioned to make this same response to the word WORM if told, "a worm is an animal." When the animal meaning response has become anticipatory to the word WORM in the manner already described, the word would be fully in the concept verbal habit-family.

Following Mowrer's conception the sentence would also condition the meaning of ANIMAL to the meaning of WORM, i.e., form an rm_w–sm—rm_a association. Thus, the object worm (and any object eliciting rm_w, e.g., caterpillar) would now elicit the animal meaning response and so be in the animal class of objects.

It is also likely that backward conditioning would broaden the animal concept through the same sentence by conditioning the meaning of worm to the word ANIMAL and its meaning. This would generalize to the other animal words. Dostálek (1959) discusses the importance of backward conditioning in verbal learning.

Whether or not this backward conditioning takes place, the concept animal could be further broadened on the basis of language conditioning. For example, the sentence, "animals consume oxygen," would condition the responses elicited by the predicate of the sentence to the meaning response elicited by the word ANIMALS, with the expected generalizations occurring.[6]

Thus, with continued verbal experience, the verbal habit-family (concept) grows in terms of the objects and words which elicit the concept meaning. In addition, the concept meaning is broadened to include parts of the response made to new objects and words.

On the basis of verbal habit-families and language conditioning and generalization, learning which is originally derived from experience with a relatively small class of objects, usually having identical elements, may be transferred to many new situations and tasks.[7] The child first learns the "triangle" verbal habit-family on the basis of direct experience with a few stimulus objects, i.e., on the basis of the common stimulus elements and the common responses they elicit. Later, however, he is told that three dots, or three people, are a TRIANGLE; and through the meaning which has previously been conditioned to the word TRIANGLE, the concept meaning, the new objects, and other objects which are similar to them

[6] It is also true that the concept word (i.e., ANIMAL) in a concept could gain its meaning on a language basis rather than through being paired with the various objects in the original stimulus class. That is, it should be possible for the concept word to gain its meaning through being paired with subordinate words in the verbal habit-family which had already been conditioned to their meanings. Or the concept word could be presented in sentences with appropriate adjectives and the meanings of the adjectives conditioned in this manner to the concept word. These processes would be higher-order conditioning.

[7] The class of stimulus objects in a concept usually has identical elements, but the same process could start with just one object and through conditioning meaning to meaning as in Mowrer's communication paradigm, grow to a class of objects which elicit a common meaning response and a class of words, etc.

enter the concept class. It is suggested that these are the processes which underlie the progression from concrete to abstract thinking which has frequently been said to occur in child development (Brown, 1958), and which are involved in "understanding" a concept.

Osgood was correct in stating that a rat cannot understand a concept—not, however, because it cannot form a common response to a class of stimulus objects. This part of concept learning the animal would be capable of. However, the rat is not capable of acquiring verbal habit-families to correspond to such concept mechanisms and the power of abstraction is thus lost to the animal. The process of concept formation is seen as one which involves complicated principles of learning, communication, and mediated generalization. The relationships between the language processes and the environmental process are complex. The language processes arise from response to the environment but then in turn effect response to other aspects of the environment.

Actually, concepts would not usually be as simple as portrayed. Figure 6 makes one elaboration of the animal concept to include a subconcept, the "dog" concept. Although all of the stimulus objects in the general concept elicit the common animal meaning component, rm_a, three of the objects (the dogs) also elicit a meaning component (rm_d) common to them but not the other stimulus objects. This component comes to elicit the dog word responses, including the concept word DOG itself, and is conditioned also to each of the dog word responses and becomes part of the meaning of each of these word responses. Each of these

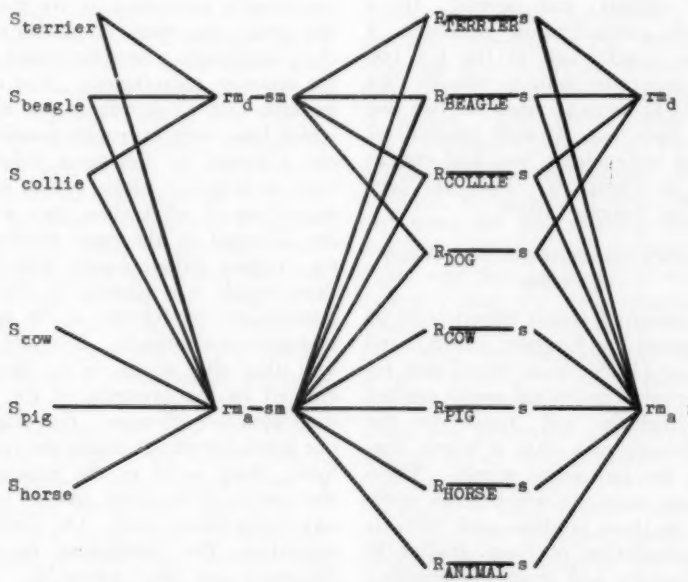


FIG. 6. This diagram depicts a concept ("dog") which is included in a larger concept ("animal").

dog stimulus objects, in addition to the rm_a and rm_d conditionable sensory response components, would also elicit a specific response component which is common to none of the other dog stimulus objects. This would also be true of the other stimulus objects. The figure is simplified by not showing these associations.

It would be expected that the greater the extent of overlap in meaning response components between any two habit-families, or any two words, the greater the amount of generalization that would occur. Thus, following Figure 6, if one said "Animals are dangerous," the meaning of DANGEROUS would be conditioned to the meaning of the word ANIMAL, and consequently equally to all of the words and objects in the figure. If, however, one said, "Dogs are dangerous," there would be greater generalization to the dog objects and words than to the other animal objects and words. In a semantic generalization paradigm, a response conditioned to the CS DOG should generalize more to TERRIER than it would to ANIMAL since the two dog words share two strongly conditioned meaning components, rm_a and rm_d as shown in Figure 6. This has been shown by Razran (1949).

OPERANT CONDITIONING OF WORD CLASSES

A number of recent experiments as summarized by Krasner (1958) and Salzinger (1959) have shown that reinforcement of individual words emitted by individuals will result in the strengthening of a class of words containing the individual words. There are some questions which seem unresolved in these studies—and there is little articulation of these studies to the principles and studies concerning the semantic properties of language. Primarily, there is not adequate rationale concerning the operant conditioning of classes of responses which are as diverse as the classes of verbal response which have been conditioned, e.g., travel words (Wilson & Verplanck, 1956), living thing words (Wilson & Verplanck, 1956), animal words (Ball, 1952), action words (Wickes, 1956). In his article on the conditioning of verbal behavior, Salzinger (1959) seems to recognize the need for such a rationale in discussing the definition of verbal response classes, stating that responses in a class may be substituted for one another, are followed by the same reinforcement, or are emitted in the presence of the same discriminative stimulus.

It is not the case, however, that a word in one of the verbal response classes which has been conditioned may be substituted for any other word in the class. In language usage individual words of a word class are not functionally equivalent as are the rat's bar press responses. Substitution of class words into a sentence could make the sentence meaningless. Nor can it be said that all words in the classes which have been operantly conditioned are followed by the same reinforcement in language usage. Most words, regardless of what class they are in, are followed by the same reinforcers, e.g., tacting living objects with living thing words is reinforced by the same generalized reinforcers as is tacting transportation objects. Nor can it be said that the words in a class are emitted in the presence of the same discriminative stimulus. For example, one is reinforced for saying the specific, living thing word in the presence of the specific living thing, not for saying *any* living thing word. On closer examination, the conclusions made by Salzinger are thus untenable. Salzinger, however, is quite correct in recognizing the need to explain why

verbal response classes are strengthened when individual members are reinforced. The paradox is that as responses these words in the word classes may be very discrepant, and cannot be considered as functionally equivalent, yet response induction occurs nevertheless.

As would be expected from the above analysis, it is not possible to predict what words will operantly condition as a class from an operant conditioning rationale, as Salzinger (1959) admits:

> Generally, investigators have had to rely upon their common sense knowledge of verbal behavior to decide upon the constitution of response classes (p. 70).

He continues to suggest that studies of semantic generalization or measurements of the connotative meaning of words may be used to discover response classes. Thus, in both cases, this implicitly recognizes that the commonality of words in the classes which have been operantly conditioned concerns the semantic properties of the words. Notwithstanding this, the account of verbal behavior offered by Skinner eschews the concept of meaning and his example appears to be followed by other investigators interested in the application of operant conditioning principles to language behavior. At any rate, it does not seem to be defensible to exclude the concept of meaning in accounting for the operant conditionability of classes of word responses, and then suggest that meaning properties be used to choose classes of words which *will* condition. In conclusion, this suggests that a combination of operant principles of conditioning along with the semantic properties of words such as has been offered herein, will offer a more complete explanation of language behavior.

Osgood (1957b), while not having dealt with the operant conditioning of word responses in any detail, has briefly suggested a way of accounting for the operant conditioning of word response classes which may be further detailed using the concept of the verbal habit-family:

> In dealing with adult subjects, we must assume that these associations between mediators and appropriate vocalizations are all available and considerably overlearned. Nevertheless we can temporarily change the relative availability or probability of alternative mediators themselves by manipulating reinforcement conditions. What we observe then is a shift in the emission frequency of certain forms relative to others. This is the way I would interpret the results of many recent experiments on the operant conditioning of verbal behavior (p. 375).

This brief statement can be elaborated in necessary detail through application of the concept of verbal habit-families. In short, it is thought that word response classes which will condition are verbal habit-family hierarchies. Subjects in the verbal conditioning studies are reinforced when they utter a word from a certain class of words. The verbal habit-family (i.e., concept) depicted in Figure 6 will be used as an example. In this example, if the subject spoke an animal word the rm_a meaning response would be elicited. If a social reinforcer was then presented, it would be expected that the rm_a meaning response as well as the verbal response itself would be conditioned to the cues of the situation. The strengthening of the individual speech response would not generalize to different topological speech responses even though they were in the same class of words. However, if only animal words are reinforced, the strongest meaning association to the situation will become that of the animal meaning response component. Since the animal meaning response component is also the

anticipatory response for the class of animal words, it would be expected that the whole class of animal words would be strengthened, i.e., the frequency of their occurrence would be heightened. It is the strengthening of the common anticipatory meaning response in the verbal habit-family that mediates the generalized strengthening.

In Figure 6, which is used as the example, some of the words are in subconcept classes, which actually have aspects of separate verbal habit-families. Reinforcing a word in one of the subclasses would strengthen all the meaning responses elicited by the word. In the case of the word TERRIER, for example, both rm_a and rm_d would be strengthened. Because of this there would be a greater tendency for another word in the subconcept to be emitted than one only in the general concept. This might lead to the strengthening of classes of words other than those the experimenter had intended to strengthen, and this apparently occurs, according to Salzinger (1959):

> The problem which arises here is the discrepancy between the experimenter's definition of the response class and the response class which is actually being affected by the experimental manipulations. In one study . . . for example, the experimenter tried to condition plural nouns but actually caused an increase in a subset, i.e., names of tribes (p. 68).

This result is accounted for by the present model, i.e., the experimenter strengthened all the meaning response components elicited by the word responses he reinforced. A few reinforcements of the names of tribes could so strengthen *all* the meaning components so that only these types of responses would be elicited.

One implication involved in this discussion is that if classes of words can be operantly conditioned, then they are members of a verbal habit-family, and therefore have a common meaning response component. Thus, semantic generalization should be possible between individual members of the words in a class which can be operantly conditioned, as Salzinger has implied. And, on the other hand, any concept, or verbal habit-family, should be capable of being operantly conditioned. Thus, for example, each of the concept classes of words found by Underwood and Richardson (1956) should be capable of operant conditioning as a class since each group of words shares a common meaning response component.

The many variables concerned with the strength of the concept meaning response, the extent to which the response produces distinctive cues, the dominance of the concept meaning response in the total meaning elicited by the words in the class, and so on, should be factors in the ease of the operant conditioning of the word class.

SUMMARY

A detailed model of verbal habit-families was described which more fully utilizes Hull's habit-family conception. Additional specifications of the responses involved in verbal habit-families were made as well as of the manner of development and function of verbal habit-families.

This model was then used to indicate how concepts develop and function in a process involving complex learning, communication, and mediated generalization. Complex relationships between language processes and environmental processes were described in concept formation, i.e., language processes arise from response to the environment but then in turn effect response to other aspects of the environment. Abstraction which

is based on concept learning was seen to depend on verbal habit-families.

An adequate account of the operant conditioning of word classes was said to require knowledge of both the principles of operant conditioning and the semantic properties of word responses. Such a combined account was given throught the use of the verbal habit-family model.

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(Received December 16, 1959)

Psychological Review
1961, Vol. 68, No. 3, 205–215

COMPONENTS OF GENERAL ACTIVITY AND THE ANALYSIS OF BEHAVIOR [1]

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In studying behavior, the psychologist selects, as he must, some particular aspect of an animal's behavior for analysis. He carefully identifies the responses he will investigate (running, jumping, bar pressing, withdrawing, attacking, etc.) and defines them in unambiguous terms. Typically, the definitions involve some explicit or implicit reference to certain crucial events, called goals, such as eating, drinking, copulating, pain avoiding, and other so-called "drive activities" or consummatory responses. Such responses, defined with reference to specific goals, form the subject of most experimentation and theorizing, and it is from their analysis that the psychologist hopes to discover the laws of behavior. However, there are other activities, displayed by animals in a wide variety of situations, which cannot be as formally defined with reference to particular goals. These activities are currently subsumed under the rather vague rubric of "spontaneous" or "general" activity.

General activity is usually studied in no more refined a way than by measuring the over-all motility of the animal (e.g., in a stabilimeter or activity wheel). Such methods of measurement have obscured the fact that general activity is itself composed of a variety of specific, identifiable, motor events; these include the acts of lying, sitting, walking, stretching, changing posture, examining surrounding objects and happenings, manipulating parts of one's body, and so on. Such components of general activity normally take up a major portion of an animal's waking hours, whereas responses usually linked to specific goals are isolated, infrequent events in the life of an animal (Nissen, 1954). But, so far, the specific acts that make up general activity have received little attention from the experimental psychologist, still less from the psychological theorist.

The purpose of this paper is to show that acts comprising general activity are so pervasive that they inevitably affect those responses in terms of which the psychologist seeks to develop principles of behavior. Consequently, knowledge of the factors that determine the occurrence of the components of general activity is essential for the precise formulation of the principles. When such knowledge is used in interpreting the results of many different studies of learning and performance, it becomes possible to account for their results in greater detail than has been possible heretofore. A framework for conducting more complete behavioral analyses is suggested.

Components of General Activity as Meaningful Dependent Variables

Attempts have recently been made to classify the different components of general activity of the rat into reliably identifiable and quantifiable

[1] The research on which this paper is based was supported by the National Research Council of Canada (Grant APT 36). The author is grateful to his colleagues at the McGill Psychology Department for critically reading earlier drafts of this paper.

categories. Bindra and Blond (1958), employing a modified time-sample method, observed rats individually in an observation cage and recorded the act that was in the process of occurring at the start of each successive 6-second interval for several minutes. Each response was recorded as belonging to one of five jointly exhaustive categories: sniffing (vigorous movements of whiskers accompanied by rearing or head turning, or both), grooming (licking or scratching parts of body), walking, lying, and miscellaneous acts. With minor modifications in the number and the exact definitions of the categories to suit specific purposes, this method has been adapted for studying the components of the rat's general activity in a number of experimental situations (Bindra & Spinner, 1958; Claus & Bindra, 1960).

The record of a rat's activity obtained by noting the response in progress at each xth second during a given observation period (walking, walking, rearing, rearing, rearing, sitting, grooming, grooming, lying, etc.) can be described readily in terms of the frequency of occurrence of each response category. This measure has been found to yield high interobserver agreement, as well as high self-consistency for the animals (Bindra & Blond, 1958).

At present there is little reliable knowledge about the components of general activity in species other than the rat. However, the work of ethologists (e.g., Andrew, 1956) shows that general activity acts comparable to those of the rat can be identified and quantified in other species too. Preening in birds seems to be such an act; head turns and scanning eye movements, as well as changes in posture, appear to be easily quantifiable in primates, including man. Whatever the exact manner of development of the acts comprising general activity, normally they all seem to become well established in the repertoire of the animal early in infancy.

Some recent ethological and experimental studies have shown that certain components of general activity may be meaningfully related to the more formally defined responses of the animal. For example, Beach (1956) describes licking and "washing" as integral parts of sexual activity of the male rat. Lorenz (1935, 1941) has observed preening to be associated with fighting in certain birds. Hansen (1956) has shown that the occurrence of crouching and washing in mice systematically varies with the stage of learning a maze and with the performance of error vs. errorless runs. And Claus and Bindra (1960) have shown rearing and walking in rats to be related to the degree of transfer of a trained drinking response to a different test situation. These findings suggest that components of general activity may play an important part in the execution of many normal activities of animals, as well as their performance on experimental tasks. A general framework for studying the role of the acts comprising general activity in learning and performance is proposed in the next section.

A Framework for Behavioral Analysis

Though it is suggested more as a framework for detailed experimental analysis of behavior rather than as a model or theory, the present formulation is in some respects like the familiar response-competition model of Guthrie (1935). Two recent formal statements of such a model are those of Estes (1950) and Restle (1955). As do these models, the present formulation considers the occurrence of a response of a certain class in a given

situation to be the outcome of its (successful) "competition" with other classes of reactions, which also have a certain probability of occurrence in that situation. Further, like the Estes and Restle models, learning and extinction are looked upon as transfers of probability relations between the competing classes of reactions. An important difference between the present approach and that of Estes and Restle is that their theories relate the probability of occurrence of a defined response to the nature of the stimulus cues received by the animal, whereas the present suggestion is to relate the defined response to the more molecular motor events. The two approaches are complementary; Estes and Restle stress an analysis of sensory events, whereas I ask for a detailed analysis of motor events. Inasmuch as, at this time, it is more feasible to measure response events than sensory events, the present approach has some temporary advantage in generating empirical work in many areas of experimental psychology.

General Activity Matrix and the Required Response

Each response, as it is defined and required by the experimenter, may be looked upon as consisting of a number of sequentially arranged motor events or *acts*. Some of these acts are mainly manipulatory, such as walking, pushing, sitting, grooming, and handling objects; other are primarily perceptual, such as head turning, eye movements, and sniffing. Acts are the molecular events that make up the molar response. An act that contributes to the required response in the sense that the response cannot be completed until the act occurs in its proper place in the sequence may be referred to as a "relevant act." An *irrelevant* act does not so contribute to the response, and its elimination from the sequence only makes the response more efficient or its emergence more likely.

When an animal is confronted with a new experimental situation, the specific acts that make up its general activity may be said to constitute a matrix (*Anlage*). This matrix contains the acts comprising the defined response, as well as many acts that are irrelevant so far as that response is concerned. The performance of a defined response consists in the occurrence of each of its relevant acts at its proper place in the sequence. The acquisition of a defined response involves a progressive increase in the occurrence of each of its relevant acts at its proper place. Obviously, a response an essential component of which is not to be found in the matrix could never be "shaped" in the situation. Thus the detailed characteristics of the emergence and performance of the desired response may be expected to depend upon the particular act classes that constitute the matrix.

The outcome of competition between different acts is determined by their relative probabilities; the act with a higher probabitity of occurrence will, on the average, occur *sooner* than an act with a lower probability of occurrence. Thus the occurrence of a relevant act at a given point in the sequence comprising a defined response is determined by the probability of occurrence of that act relative to the probabilities of occurrence of other acts that form the matrix. The higher the relative probability of the relevant act the sooner will it occur. This means that the time taken in the execution of a defined response (including its latency) is the most meaningful measure of performance, for it represents the sum of the times taken by each component

relevant act in overcoming its competitor.

Though these ideas are familiar enough, for the most part they are made use of only in post hoc explanations of unexpected or discrepant results; they are seldom, if ever, used in planning experiments or as a basis for predictions. It is true that the theories of Estes (1950) and Restle (1955) imply the existence of such a matrix of acts, but their formulations do not indicate what the acts comprising the matrix are nor how their relations to the desired response might be determined. Though these theories are said to be response-competition theories, in their empirical implications they are really "stimulus-competition" ones, for they relate the required response to combinations of stimulus elements, not to the motor elements from which the response is shaped.

Act-Response Relations

As mentioned before, it is possible to identify and quantify the specific acts that comprise the general activity matrix. Thus characteristics of a matrix can be empirically defined in terms of the frequencies of occurrence of the different classes of acts and of the progressive changes in these frequencies with specified changes in the given experimental situation. Insofar as these acts are observable events, their probabilities may be empirically determined and, from these probabilities, predictions made about the defined response. This points to the desirability of studying the relations between the effects of independent variables (e.g., stimulus change, "drive," brain damage) on the probabilities of occurrence of the acts in the matrix and the effects of the same variables on a defined response (e.g., bar pressing, copulating, fighting). The establishment of such relations is likely to facilitate the formulation of general principles which would allow one to predict with confidence the effects of a given variable on any defined response from a knowledge merely of the effects of that variable on the components of general activity.

So far, the main concern of psychological theorists has been with relation between the stimulus characteristics of the experimental situation and the occurrence of a defined response. Of course, this still remains an important, perhaps the most important, problem. However, it is also meaningful to consider in detail the relations between two classes of independently defined response events: the desired, specified response; and acts that comprise the general activity matrix. As Audley (1960) has recently remarked:

> On the whole there is a certain looseness in the way in which many contemporary theories and even local hypotheses are linked to observe response variables. It seems worthwhile, therefore, to try to determine whether these [response] variables might not be related to one another by relatively simple laws . . . (p. 14).

DETAILED ANALYSIS OF BEHAVIOR IN CERTAIN EXPERIMENTAL SITUATIONS

The purpose of this section is to bring forth evidence in support of the idea that a consideration of the components of general activity that occur in an experimental situation is likely to lead to a more detailed and exact analysis of the phenomenon under investigation. This point is supported here with reference to a number of different types of studies in each of which it is shown that a consideration of general activity acts makes it possible to account for aspects of experimental results that have usually been ignored or have remained unexplained.

Each analysis suggests further experiments that would advance our understanding of the phenomenon in question.

Simple Reward Learning

Consider the learning of a simple running response by a rat. The rat, maintained, let us say, on a water deprivation schedule is placed in a starting box at one end of a runway; a water bottle is placed in a goal box at the other end. At the end of acquisition, the instrumental activity is made up of a fairly well integrated sequence of a number of component acts, starting with walking out of the starting box and ending with drinking. In the course of learning, sitting motionless or "freezing," sniffing, grooming, and other novelty reactions [2] have dropped out, resulting in the sequence of integrated acts that constitutes the required (running) response. Many of the acts (e.g., walking, drinking) in the final sequence did, of course, exist in the animal's repertoire before the experiment, so that the "learning" of the required response consists in the gradual elimination of those components of the general activity matrix that do not contribute to the response. Conversely the extinction of the response consists in the gradual increase in the occurrence of irrelevant components.

Now, there can be no doubt that in the above example the emergence of the final running response is dependent upon the factors that control the occurrence of the walking, sniffing, grooming, sitting motionless, and other acts comprising the matrix. The greater the frequency of occurrence of the relevant component acts in the matrix the more likely it is that the response will be shaped. For example, increase in freezing brought about by a sudden loud noise, will decrease the frequency of occurrence of locomotory acts and, thus, will delay the emergence of the running response. Indeed, frequently in such experiments some of the animals have to be discarded for the frequency of freezing in them increases rather than decreases with time. It is also well known that the emergence of a running response may be facilitated by simultaneously putting two or three animals in the runway on a preliminary trial. In a new situation, the rat tends to be much more active in a group than alone, and the increased occurrence of locomotory responses on the preliminary (group) trial carries over into the later individual training trials. Conversely, the extinction of a running response may be facilitated by any arrangement that would evoke some "other," irrelevant acts in the situation. In general, whatever conditions increase the frequency of occurrence of the relevant acts in the running response facilitate the emergence of the response and increase its resistance to extinction; whatever conditions decrease the frequency of such acts delay the emergence of the response and decrease its resistance to extinction. Though some such principle as this has been implied by some authors (e.g., Karn & Porter, 1946), it has been visualized more in terms of the facilitatory and inhibitory effects of prior experiences of the animals rather than in terms of the fre-

[2] It should be clear that the term "exploration" is inappropriate for describing all the different responses which occur under conditions of stimulus change. Freezing and grooming could hardly be described as exploration. If a general label is needed to describe the alterations in the incidence of the components of general activity in response to stimulus change, a more suitable term would be "novelty reactions" (Bindra, 1959).

quencies of occurrence of observable relevant and irrelevant acts during the experimental trials.

By way of an example, apply the above considerations to an analysis of the obtained differences in speed of maze learning between animals reared in a "free," enriched, environment and those reared in a restricted environment. Such differences have been found in experiments with rats (Forgays & Forgays, 1952; Hymovitch, 1952) and dogs (Thompson & Heron, 1954). Zimbardo and Montgomery (1957) suggest that the maze learning superiority of the free-environment animals may be due to the fact that the test situation is relatively less novel for them, being more like the "free environment cages" than are the "restricted cages," so that during maze learning they engage in less "exploratory" activity (locomotion) and thus make fewer errors. Recently Woods (1959) and Stewart (in press) have provided strong evidence in favor of this view. Woods was able to attribute the large error scores made by his restricted animals at least partly to the greater frequency of occurrence of the locomotory acts. A greater increase in locomotion in response to any new situation has also been reported in the case of dogs reared in a restricted environment. Clarke, Heron, Fetherstonhaugh, Forgays, and Hebb (1951) found that:

> If taken into an unfamiliar room repeatedly . . . there was a steeper falling-off in the activity level of the free-environment animals. These dogs spent a significantly greater amount of time sitting and lying down after a number of exposures to the room than did the restricted (p. 153).

It is likely that the inferiority of "restricted" dogs in problem solving (Thompson & Heron, 1954) is also attributable, at least in part, to the greater incidence of the interfering novelty reactions. It seems reasonable to suppose that, in developing strains of maze-bright and maze-dull animals, one is to a certain extent breeding for differences in "emotionality" or "shyness" or whatever characteristic of the animal determines the extent of its locomotion in a mildly novel situation. If this suggestion is correct, it would seem desirable to undertake a series of experiments designed to analyze the relation between novelty reactions and maze-brightness.

Avoidance Training

Studies of avoidance training report marked individual differences in the ease of acquisition of an avoidance response. For example, if the response is one of walking at a given signal, before the onset of an electric shock, from one side of a shuttle box to the other, some animals may learn the response within 20 trials, others in 70 trials, and still others may have to be discarded for failing to acquire the response in 200 trials. Though this depends upon the exact procedure employed, it is not uncommon for about 40% of the animals to be discarded from an experiment for failing to acquire the avoidance response (e.g., Black, 1958; Kamin, 1959). No formal explanation for the necessity of such discards has yet been offered.

It follows from the present formulation that, in the shuttle box type of situation, the factors that increased the incidence of walking will facilitate learning; those that decrease walking will delay, or completely preclude the possibility of, the emergence of the avoidance response. The major factors that are likely to decrease walking in the situation are the extreme novelty of the situation and the electric shock, both of which often evoke freezing. Consistent with this statement is

the common observation that animals that fail to learn the avoidance response are those that show very little walking during the pretraining habituation period, as well as during the intertrial periods. And these tend to be the same animals whose initial response to the shock is that of freezing rather than jumping around, so that they take longer even to escape the shock. It is also well known that, by spacing the training trials over a number of days and thus decreasing freezing and increasing walking more than would be the case under massed trials, one can facilitate the emergence of the avoidance response. Thus, by giving a preliminary habituation period and only 20 trials a day to the rat in a shuttle box, it is possible to train almost all animals within 80 trials (Stern, 1960). Another way to facilitate the emergence of the response would be to place two or three animals in the box simultaneously during the pretraining habituation period; as noted before, a group situation tends to decrease freezing and increase locomotion. However, during extinction, the irrelevant "social" reactions evoked by the presence of other animals are likely to facilitate extinction of the response. Experiments could be designed not only to test these qualitative predictions, but to establish quantitative relations between the frequencies of occurrence of relevant and irrelevant acts and the ease of acquisition and extinction of the defined response.

Further support for the idea that the performance of an avoidance response depends upon the extent to which the components of general activity exhibited by the animal in the situation are synergistic with or antagonistic to the to-be-trained response comes from an elegant experiment by Myers (1959). In this experiment, rats were trained to press a bar on the presentation of the CS (tone or buzzer) in order to avoid an electric shock. One of Myers' findings was that, with buzzer CS, rats learned better than they did with tone CS. Myers attributes these differences to the contributions made by the components of general activity of the animals. The typical reaction of the rats both to tone and buzzer is freezing; however, in the case of buzzer the freezing is preceded by a startle response. Myers points out that startle in the case of buzzer was sufficient to operate the bar, when the rat was allowed to hold the bar between trials. This analysis in terms of molecular acts led Myers to a further experiment. In the second experiment, he was able to show that the difference between tone and buzzer was greatly reduced when the bar was electrified, thus preventing the animals from holding the bar between trials. Myers interprets his other findings pertaining to strain differences, and differences between day and night performance, in a similar way, with reference to the effects of these conditions on the occurrence of the relevant acts. This and other similar experiments (e.g., Meyer, Cho, & Wesemann, 1960) clearly point to the need for systematically considering the components of a defined response in planning investigations of the determiners of avoidance behavior.

Effects of Stimulus Change

Apart from the many studies in which the novelty introduced into the experimental situation is an unwanted confounding variable, there are investigations in which it is the psychologist's purpose to determine the effects on a given response of a specified stimulus change. The importance of novelty reactions in these types of

studies can be best illustrated with reference to "transfer" experiments. In these experiments, the investigator studies the changes in response (typically response decrement) brought about by a change in the stimulus characteristics (e.g., auditory, visual, tactual) of the experimental situation.

According to the present formulation, the response decrement, seen when a response acquired in one situation (S) is tested in another, similar but different, situation (S′), is attributable to an increase in the frequency of occurrrence of some irrelevant components of general activity evoked by the novelty provided by the new situation. For example, the increase in sniffing or freezing evoked by the stimulus change would interfere with a trained running response. If this is so, the degree of response decrement should be proportional to the extent to which such novelty reactions are evoked. This has been shown to be true for trained drinking (Claus & Bindra, 1960), fighting (King & Gurney, 1954), and running (Bindra & Seely, 1959) responses.

It also follows from the present formulation that conditions that reduce the novelty of a test situation should also reduce response decrement. Prior exposure to (i.e., familiarity with) the test situation is one such condition. Bindra and Claus (1960) have shown that a group of rats exposed to the test situation *before* training in a bar pressing response showed less response decrement than a group without such pre-exposure when, after acquisition of the response in a similar but different training situation, the response output was measured in the test situation. In a more quantitative study, Claus and Bindra (1960) have shown that the number of types (visual, auditory, tactual) of stimulus components of the test situation to which an animal is "pre-exposed" is inversely related to both the incidence of novelty reactions (sniffing-walking) and the degree of decrement in a drinking response. As shown elsewhere (Bindra, 1959) these results, while they are consistent with the present formulation, would not, without making further assumptions, be predicted from the current statements of learning theories such as those of Hull (1943), Bush and Mosteller (1951), and Estes and Burke (1953).

The above evidence suggests that novelty reactions would contribute at least to some extent to the results of experiments on "stimulus generalization." These experiments typically generate gradients of habit strength which decline monotonically as they are evoked by test stimuli that are progressively further removed, along a given stimulus dimension, from the training stimulus. Evidence cited above would indicate that this response decrement is due, at least in part, to the irrelevant acts evoked by the change from the training to the test stimulus; the further removed the test stimulus is from the training stimulus, the greater the evoked novelty reactions, and hence the greater the response decrement. Thus, if there is a true gradient of *primary* stimulus generalization, as visualized by Pavlov (1928) and Hull (1943), it must be less steep than what is reported in the literature, for at least a part of the decrement in response would seem to be attributable to the increase in the frequency of irrelevant novelty reactions evoked by the stimulus change. The exact contribution of novelty reactions in producing the gradients of habit strength in stimulus generalization is one research problem suggested by the present analysis.

Effects of Drugs on Behavior

According to the present view, the effect of any particular drug on a given response would depend upon the drug's influence on the frequencies of occurrence of components of general activity and the component acts comprising the defined response. As we have seen, in the acquisition of most responses it is, in general, necessary for the animal to be quite active when it is first introduced into the learning situation, so that the acts comprising the desired response may have a chance to occur. Thus most drugs that reduce general activity (depressants) would tend to delay the acquisition of a response and drugs that increase general activity (stimulants) would facilitate acquisition. By and large this is what happens both during the acquisition and, later, during test performance and extinction of the response (see, e.g., Hilgard & Marquis, 1940, pp. 119f.). However, when the desired response is one which requires the animal to be inactive rather than to move around, then the effects of the two types of drugs would be expected to be reversed. This has been shown to be true at least for one response. Bindra and Baran (1959) have found that the "tranquilizer" chlorpromazine reduces the incidence of sniffing (including walking and rearing) but increases the frequency of sitting in the rat, and Blough (1958) has shown that chlorpromazine *improves* the performance of the pigeon when the required response is that of standing still in a particular location (i.e., reinforcement is contingent upon the bird's standing in one place).

Thus the kind of drug induced effect obtained depends upon the acts comprising the required response and upon the effect of the drug on the occurrence of those component acts. These findings again point out to the desirability of a more detailed analysis of the defined response and of considering the acts that comprise the response in relation to the acts in the general activity matrix.

SUMMARY

Eating, drinking, copulating, avoiding pain, and other so-called "drive activities" or consummatory responses are isolated, infrequent events in the life of an animal; the specific acts that make up what is called "general" or "spontaneous" activity occur far more frequently. Indeed, the acts comprising general activity are so pervasive that they inevitably affect an animal's other, more formally recognized, responses. This paper attempts to show that knowledge of the factors that determine the occurrence of acts comprising general activity in a species is essential for a precise analysis of any segment of its behavior.

Ethological studies, as well as some recent psychological ones, show that it is not difficult to classify the components of general activity of different species into clearly identifiable and quantifiable categories. For example, walking, grooming, rearing, lying, sitting, and sitting motionless (freezing) are component acts of general activity of the rat that form easily identifiable categories. By employing a time-sample method of recording, it is possible to estimate reliably the frequency of occurrence of each category.

When an animal is confronted with a new learning situation, the general activity acts it displays serve as a matrix (*Anlage*) which provides the relevant acts necessary to make up the required (to-be-trained) response. It

is suggested that the detailed characteristics of the emergence and performance of any particular defined response (e.g., bar pressing, copulating, fighting) depend upon the frequencies of occurrence of the (relevant) acts comprising the required response. Thus, the probability of occurrence of a defined response as a whole is linked to the probabilities of occurrence of the relevant and irrelevant components of the general activity matrix. It follows that the exact change in the performance of any given response brought about by a particular experimental manipulation (e.g., brain damage, stimulus change, drug injections) can be fully understood only if the effects of that manipulation on the occurrence of the components of general activity are known. A more "molecular" analysis of the response and general activity is indicated.

The proposed formulation differs from the theories of Estes and Restle in that they stress an analysis of sensory events, whereas this author emphasizes the need for a detailed analysis of motor events. The present formulation provides a frame work for a systematic study of the relations between two levels of (observable) behavioral events, between "molecular" acts and defined "molar" responses. It also suggests a way of making predictions from the effects of an independent variable on the components of general activity to its probable effect on any defined response whose component acts are known. The advantage of adopting this formulation is that it enables one to account for those aspects of experimental results that have usually been ignored or have remained unexplained. This is shown by considering in detail the applicability of the formulation to experiments in such diverse fields as simple reward learning, avoidance training, transfer and stimulus-generalization studies, and investigations of the effects of drugs on behavior.

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(Received December 30, 1959)

Psychological Review
1961; Vol. 68, No. 3, 216–218

THEORETICAL NOTES

A NOTE ON THE THRESHOLD FOR EMOTIONAL STRESS [1]

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A wide range of noxious stimuli has been shown (Selye, 1950) to precipitate release of adrenocorticotrophic hormone (ACTH) from the anterior pituitary, and consequent release of adrenal cortical hormones (corticosterone, hydrocortisone) from the adrenal medulla. But virtually all stimuli that release ACTH also increase sympathetic nervous activity and lead to release of adrenaline and noradrenaline from the adrenal medulla (Ramey & Goldstein, 1957). These two responses may therefore be considered, as Cleghorn (1953) suggests, the two arms of a single response to stress.

Both arms of the response to stress are, in general, mediated by the posterior and medial hypothalamus, extending from the mammillary bodies forward to the infundibulum (see Bovard, 1958). This is particularly the case for emotional or neurogenic stress, that is to say, stress that must necessarily be mediated by the central nervous system.

The threshold for emotional stress is therefore determined, in large part, by the level of electrical activity obtaining in the posterior and medial hypothalamus. This level of activity, in turn, is itself influenced or modulated by neural activity elsewhere in the brain.

First of these influences is amount of anterior and lateral hypothalamic activity in progress. The work of Shimazu, Okada, Ban, and Kurotsu (1954), together with the work of Redgate and Gellhorn (1955), and Gellhorn, Nakao, and Redgate (1956) suggests that an increase in anterior and lateral hypothalamic activity will inhibit firing of posterior hypothalamic neurons. Therefore any stimulus, such as handling (Bovard, 1958), that stimulates anterior hypothalamic activity, will thereby dampen posterior activity and hence raise the threshold for emotional stress.

A second influence on the posterior hypothalamic zone is projection of the hippocampal-fornix system and the amygdaloid complex to the hypothalamus (see Bovard, 1958, Figure 1). Mason and his colleagues have shown that, in monkey, the hippocampal-fornix system [2] (1959a) acts to dampen the pituitary-adrenal response to prolonged emotional stress, thus protecting the animal against the protein catabolic consequences thereof, while the amygdala (1959b) normally operates in this animal to boost the pituitary-adrenal response to emotional stress.

In addition to these two neural influences on the posterior hypothalamus, and hence on the pituitary-adrenal response to stress, there is a third and chemical influence on the pituitary-adrenal response which may or may not be mediated by the hypothalamus. Continuous production of ACTH from the

[1] This paper was originally presented at the graduate seminar in neuroanatomy, neurophysiology, and neurochemistry, Montreal Neurological Institute, April 25, 1960. I should like to thank Peter Gloor of the institute for his helpful criticism of the ideas put forward here. This paper was made possible by a United States Public Health Service special research fellowship, sponsored by the National Institute of Mental Health, at Montreal Neurological Institute.

[2] The notable increase in fornix fibers from the monkey brain to man, pointed out by Elizabeth Crosby (personal communication, 1960), could be accounted for on the assumption that such an increase has biological value in dampening the increased emotional stress experienced in his human environment by man.

anterior pituitary and consequent production of adrenal steroid hormones from the adrenal cortex, leads to an increase in ACTH and adrenal cortical hormone blood levels. In turn, higher blood levels of ACTH (Kitay, Holub, & Jailer, 1959) and of adrenal cortical hormones (Peron & Dorfman, 1959), resulting in this case from exogenous administration of these hormones, have been shown to inhibit further release of ACTH from the pituitary.

Under prolonged emotional stress, therefore, such as bombing in the London blitz, continued peak output of the pituitary-adrenal system and resultant protein catabolic effects would be prevented by both the hippocampal-fornix and chemical negative feedback systems.

Two further consequences of the neuroanatomical arrangements that mediate emotional stress are worth noting here.

First, assume an emotional stress, like a forthcoming doctoral preliminary examination. This stimulus, we can further assume, will increase the level of posterior hypothalamic activity, thus lowering the threshold for other noxious stimuli, such as one's wife not having supper ready on time. To take another example, a session on the carpet with the boss, resulting in a later outburst at the children for being too noisy, can therefore be interpreted not as displacement of a charge of energy from an appropriate stimulus to an inappropriate one, but rather as the effect of emotional stress, through posterior hypothalamic mediation, in lowering the threshold for ordinarily innocuous stimuli.

Second, let us assume a stimulus that arouses anterior and lateral hypothalamic activity. This would, according to the position taken here, then dampen or inhibit posterior activity. The finding that scratching a dog behind the ears or mere presence of someone lowers the dog's heartbeat (Gantt, Newton, & Stevens, 1960) could therefore be accounted for on the assumption that handling or social stimuli (Bovard, 1959) activate the anterior hypothalamus, or functionally analogous region of the hypothalamus, and thereby inhibit sympathetic output to the heart. Harlow and Zimmerman's finding (1959) that presence of a cloth "mother" inhibits the infant monkey's fear response to a strange situation, could also be accounted for on the basis that this stimulus dampens posterior hypothalamic activity. A simple test of these hypotheses would be stereotaxic placement of lesions in anterior hypothalamus of dog or monkey to see whether the dampening effect of social stimuli would thereby be eliminated.

These considerations suggest the effects of emotional support in psychotherapeutic, family, and military situations could be accounted for on the assumption that the social stimuli involved, probably through anterior-lateral hypothalamic mediation, dampen posterior hypothalamic activity and hence raise the threshold for emotional stress.

Whether a given psychological stimulus is noxious or not, is determined at higher levels in the nervous system, perhaps in the amygdaloid complex[3] of the temporal lobe, as the work of Gloor (1955a, 1955b) and others has suggested. But the extent to which this or any other neurally mediated stimulus triggers a protein catabolic response in the vertebrate organism depends, in the final analysis, on the level of electrical activity in progress in the posterior and medial hypothalamus, together with blood stream levels of ACTH and adrenal cortical hormones.

[3] One problem here is how the amygdaloid complex, with its apparent lack of any topographical organization (for example, stimulation of adjacent points in the amygdala produces a rise and lowering of blood pressure, respectively), can modulate the hypothalamus and its reciprocally inhibitory anterior and posterior zones. Projection of the amygdala to the hypothalamus appears to be most directly to anterior hypothalamus (P. Gloor, personal communication, 1960). A theory accounting for such amygdaloid modulation of the hypothalamus in terms of neural circuits involved has been developed recently by Gloor (1959).

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(Received August 3, 1959)

Psychological Review
1961, Vol. 68, No. 3, 219–224

NONRANDOM STIMULUS SAMPLING IN STATISTICAL LEARNING THEORY [1]

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It generally is assumed in statistical learning theory (Estes, 1950; Estes & Burke, 1953) that the subset, s, of elements selected by the subject on any conditioning trial constitutes a random sample from a larger set of elements which are relevant to the conditioning situation. The purpose of this paper is to investigate the properties of one kind of nonrandom stimulus sampling. Specifically, concern will be accorded the situation wherein the number of elements common to s on Trials n and $n + 1$ varies inversely with the amount of time elapsing between trials.[3] In order that the reader more easily follow the development, background concepts in statistical learning theory will be reviewed briefly.

Associated with an acquisition or extinction session is a finite set of stimulus elements, S, from which the subject samples, on each trial, a random subset, s. Explicitly, each stimulus element in S is assumed to have an independently equal chance of being sampled by the subject on each trial. If the trial is a reinforced one, then the two elements in s become conditioned to the reinforced response; and if it is an extinction trial, the elements become deconditioned (conditioned to another response). Probability of response is defined by the proportion of elements in S that is conditioned to the response. Since s is a random sample from S, the expected proportion of conditioned elements present in the subject's sample is the same as the proportion in S.

Under such assumptions the acquisition of a learned response is described easily. Because few elements will have been conditioned before the conditioning period, the first-trial increment in response probability approximates the ratio s/S. On the second trial, fewer additional elements will become conditioned to the reinforced response because some in s will have been conditioned on Trial 1. Thus, the increment in response probability decreases on each trial, and the performance curve is negatively accelerated.

Stimulus Fluctuation

Estes (1955a; 1955b; 1959, p. 424) introduced and explored the concept of stimulus fluctuation as a supplement to the earlier fixed-population, stimulus sampling model (1950). It was assumed that the composition of the stimulus set from which the subject samples on each trial may be modified from one experimental period to another, due primarily to imprecise control over the environmental context. Such modifications in the composition of S were assumed to be time-dependent, and were, hence, considered to be a source of performance changes labeled spontaneous recovery and spontaneous regression.

Estes (1955a, 1955b) gave his general theory of stimulus fluctuation a mathematical form of expression appropriate for longer time intervals, with special reference to temporal effects between periods rather than between trials.[4] He

[1] A grant from the National Science Foundation facilitated this paper.

[2] The author is indebted to Robert Radlow for a critical reading of an earlier version of this manuscript.

[3] Burke and Estes (1957), Witte (1959), and Straughan (1956) developed models incorporating nonrandom stimulus sampling. The former two are discrimination learning models based on fading stimulus traces from prior reinforcing events, but provide no differential time predictions for the conditioning paradigm herein investigated. The latter, although fitting the paradigm, does not address directly the variable of time between trials or between sessions.

[4] This is because the equations derived by Estes (1955a, 1955b) require, for solution, that sufficient time pass between events to insure that the numbers of elements in S and

assumed that the total number of elements associated with the entire experimental situation could be partitioned into two components: those, S, available to be sampled during a particular experimental period, and those, S′, not available for sampling at that time. Between periods, during discrete time increments, all elements in S possess the same probability, j, of escaping from S into S′; and all elements in S′ possess the same probability, j', of escaping from S′ into S. Spontaneous recovery and spontaneous regression are thus interpreted to be a result of stimulus fluctuation between two stimulus sets which interact between conditioning periods. For purposes of developing general equations for a series of acquisition and/or extinction periods, it was required, for mathematical simplicity, that p (probability of response) be unity at the end of an acquisition period and zero at the end of an extinction period.[5]

NONRANDOM STIMULUS SAMPLING

The present development constitutes an adaptation of stimulus fluctuation theory to the trial-by-trial sampling behavior of the subject. Observe that this is a change in the kind of stimulus fluctuation investigated. Whereas Estes (1955a; 1955b; 1959, p. 424) and McConnell (1957, 1959) posit changes in S to result from stimulus fluctuation in and out of S, emphasis here is upon interchange between s and S, or upon continuous change in the subject's sample.

While it will be evident that the concepts herein investigated are adaptable to complex discrimination contexts which may require sampling from a number of stimulus sets (see, e.g., Anderson, 1960; Burke & Estes, 1957; Restle, 1955), the discussion will be confined to those situations in which the subject is sampling from a single set, S, of fixed size.

Consider that s is a random sample of elements from S only on the first conditioning trial. Between trials each element in s has a probability, k, of fluctuating out of s and into the set S − s (i.e., the set composed of those elements of S which were not sampled on the immediately preceding trial). Similarly, those elements in S − s each have a probability, k', of fluctuating into s. Thus, on the next trial, s is composed of that portion of element which has not fluctuated out of the sample, plus a random sample of elements from S − s. If time (t) between trials is very short, successive samples will be highly similar in their composition; but, as intertrial interval increases, the number of elements common to successive samples decreases. In the limit, as t increases without bound, the composition of s approaches being a random sample of S.

At the level of the subject, the implication is that the stimulus complex to which a subject responds on Trial n is related in more than a chance fashion to that to which he responded on Trial n-1. As more time elapses between trials, the relationship between stimulus complexes on Trials n and n-1 decreases. Unless there is experimental manipulation, the subject is conceived of as systematically sampling "segments" of S as a function of time, the "segments" overlapping in degree inversely related to the amount of time between trials.

MATHEMATICAL STATEMENT

We require the following definitions:

- S = population of stimulus elements present during an experimental period
- s = set of elements sampled by the subject on a trial
- s′ = S − s
- N = the number of elements in s
- N' = the number of elements in s′
- x = the number of conditioned elements in s
- x' = the number of conditioned elements in s′

S′ have stabilized. Tests of this particular model have been confined to periods separated by 24 hours (see, e.g., Estes, 1959, pp. 424f.).

[5] McConnell (1957, 1959) has since extended the model by permitting fluctuations between S and S′ to occur during the experimental period.

k = probability (instantaneous) that an element in s will fluctuate into s′

k' = probability (instantaneous) that an element in s′ will fluctuate into s

P_n = proportion of conditioned elements in s at the end of the nth trial

P_n' = proportion of conditioned elements in s′ at the end of the nth trial

The assumptions are generally the same as those made in statistical learning theory, including those of fixed N and N'. However, two simplifying assumptions are added: k is fixed and equal for all elements in s, and k' is fixed and equal for all elements in s′.[6] The additional assumptions provide that $Nk = N'k'$ at all times.[7] Contingent upon presentation of appropriate stimulation, the proportion of conditioned elements in s determines momentary response probability.

Since, in general, the proportions of conditioned elements in s and s′ are not the same (e.g., at the termination of the first trial, all elements in s are conditioned, but, most likely, this is not true of s′), the interchange of elements between s and s′ results in a modification of the proportion of conditioned elements in s, hence modifying momentary response probability. In any diminishingly small time increment, kx conditioned elements leave s and $k'x'$ enter s from s′. Stated in differential form, the change in the number of conditioned elements may be expressed: $\Delta x = -kx + k'x'$. Dividing through by N yields the incremental change in proportion of conditioned elements in s:

$$\begin{aligned}\Delta(x/N) &= -kx/N + k'x'/N \\ &= -kx/N + k'x'N'/NN'\end{aligned}$$

Substituting Nk for $N'k'$:

$$\begin{aligned}\Delta(x/N) &= -kx/N + kx'N/NN' \\ &= -kx/N + kx'/N'\end{aligned}$$

Employing the notation $p = x/N$ and $p' = x'/N'$:

$$\Delta p = -kp + kp' \qquad [1]$$

Similar analysis of the change in number of conditioned elements in s′ yields:

$$\Delta p' = k'p - k'p' \qquad [2]$$

Treating stimulus fluctuation between s and s′ as a continuous function of time, Equations 1 and 2 become a system of linear, first-order differential equations,[8] and can be expressed:

$$\begin{aligned}dp/dt &= -kp + kp' \\ dp'/dt &= k'p - k'p'\end{aligned}$$

This system of equations can be solved by standard techniques (see, e.g., Leighton, 1952, pp. 105f.) to yield:

$$p = p_n + (p_n' - p_n) \times (1 - K)(1 - e^{-(k+k')t}) \qquad [3]$$

$$p' = p_n + (p_n' - p_n) \times (1 - K + Ke^{-(k+k')t}) \qquad [4]$$

where $K = k'/(k + k')$. Equations 3 and 4 are expressions for p and p' at any time t following a trial or experimental period, and can be solved when p_n and p_n' are known.

Having statements for the modification of p and p' between trials as a function of time, we desire expressions, $p_{n,t}$ and $p_{n,t}'$, for the proportions of conditioned elements in s and s′, respectively, t time after the nth trial. Because the mathematical development parallels that of Estes, the reader is referred to his paper (1955a, p. 373) for the method by which

[6] The assumptions of fixed N, N', k, and k', while more stringent than those of Estes's derivations, do permit application of the present model to short time intervals.

[7] Note that: $Nk = N'k'$; $N/N' = k'/k$; $(N'/N') + 1 = (k'/k) + 1$; $(N + N')/N' = (k' + k)/k$; $k/(k + k') = N/(N + N') = s/S$. The last term represents the growth constant, θ, present in earlier statistical models (Estes, 1950; Estes & Burke, 1953).

[8] Because it appears plausible to consider a subject as continuously interacting with his environment, the time functions were obtained by solving the differential, rather than the difference, equations.

$p_{n,t}$ and $p_{n,t'}$ were derived. The solutions are:

$$p_{n,t} = 1 - (1-K)(1-F(0)) \times (1-K+Ke^{-(\theta+\lambda')t})^{n-1} \times (1-e^{-(\theta+\lambda')t}) \quad [5]$$

$$p_{n,t'} = 1 - (1-F(0)) \times (1-K+Ke^{-(\theta+\lambda')t})^{n} \quad [6]$$

where $F(0)$ is the proportion of conditioned elements in S at the beginning of the session.

By a similar analysis the equations for extinction trials are:

$$p_{n,t} = F(0)(1-K) \times (1-K+Ke^{-(\theta+\lambda')t})^{n-1} \times (1-e^{-(\theta+\lambda')t}) \quad [7]$$

$$p_{n,t'} = F(0) \times (1-K+Ke^{-(\theta+\lambda')t})^{n} \quad [8]$$

When t is permitted to grow large, Equations 5 through 8 become the difference equation forms of Estes's original model (1950).

EFFECTS OF NONRANDOM STIMULUS SAMPLING

Some of the predictions yielded by the model are listed below.[9]

Within Acquisition Period

1. Initial massed-trial performance is superior to initial spaced-trial performance. Unless trials are highly massed and the ratio s/S is low, the effect is relatively transient, operating only over the first several trials.

2. Following the brief period of relative depression, distributed-trial performance becomes greater than that of massed-trial. In the limit, however, asymptote is independent of the amount of overlap in s on successive trials.

Between Acquisition and Extinction Periods

1. Unless conditioning is complete, the amount of spontaneous regression is an increasing exponential function of time since the end of acquisition.

2. As the number of acquisition trials increases, the amount of spontaneous regression decreases exponentially with asymptote of zero.

3. As acquisition intertrial interval increases, the amount of spontaneous regression decreases to an asymptote of zero with extreme trial spacing.

Within Extinction Period

1. Unless conditioning is complete, spaced acquisition training yields greater resistance to extinction than does massed acquisition training.

2. Initial resistance to extinction is greater when extinction trials are distributed. As in the case of acquisition trials, this effect will operate only over the first several nonreinforced trials.

3. After the initial extinction trials, resistance to extinction is greater with massed trials.

4. The total number of responses in complete extinction is independent of the amount of time elapsing between acquisition and extinction sessions.

Between Extinction Period and Spontaneous Recovery Test Trials

1. If $p_{n,t} < p_{n,t'}$ at the end of extinction training, spontaneous recovery is an exponential function of time since the end of extinction.

2. If $p_{n,t} > p_{n,t'}$ at the end of acquisition and $p_{n,t} < p_{n,t'}$ at the end of extinction training, the amount of spontaneous recovery is inversely related to the acquisition intertrial interval.

3. If $p_{n,t} < p_{n,t'}$ at the end of extinction, the amount of spontaneous recovery is inversely related to the extent of extinction intertrial interval.

4. The amount of spontaneous recovery is inversely related to both the number of acquisition trials and the number of extinction trials.

DISCUSSION

The model herein developed provides an interpretation of time-related phenomena as the result of nonrandom, time-

[9] Similar qualitative predictions follow from Estes (1955a, 1955b) and McConnell (1957, 1959). Predictions related to the massing of individual trials have a counterpart in the Estes model in the massing of *blocks* of trials.

related stimulus sampling. A survey of the literature for studies which meet the specifications of the model resulted in but a limited number of findings relevant to the predictions made above. While many studies have been reported wherein intertrial interval and/or number of training trials were the independent variables, for two reasons all of them are not referenced. First, it would appear inappropriate to cite as indirect "support" of a model those studies for which inhibition is, at present, satisfactory in accounting for the data. Second, often the predictions of the model hinge upon a single test-trial performance after a rest interval, but data were generally recorded in blocks of 5, 10, or 20 trials, hence confounding the results with effects of further training.

In several instances, however, the model provides predictions in a direction opposite to what might be expected from inhibition. First, Cotton and Lewis (1957) and Howat and Grant (1958) obtained a performance crossover in extinction as a function of intertrial interval. That is, resistance to extinction was greater initially with distributed trials, later with massed trials.

While no tests of significance were made, the data of Baron (1952), Prokasy, Grant, and Myers (1958), and Spence and Norris (1950) suggest a tendency for the initial block of massed training trials to result in relatively superior performance. What appears to be inhibition usually severely depresses performance under massed training conditions, as it did following the initial data points.

Finally, Cotton and Lewis (1957), Howat and Grant (1958), Ellson (1940), Lewis and Cotton (1959), Scott and Wike (1956), Sheffield (1950), Stanley (1952), and Wilson, Weiss, and Amsel (1955) report greater resistance to extinction when extinction trials are massed rather than spaced. Lewis (1956), on the other hand, obtained no difference in resistance to extinction as a function of distribution of extinction trials following training on a 100% reinforcement schedule. Stanley (1952), when employing the number of correct responses as a dependent variable, found greater resistance to extinction with distributed extinction trials.

Experimental tests of the model are not easy. Consider the data of conditioning. The effects of many already discovered variables in the classical conditioning situation would tend to obscure variance attributable to the sampling behavior of the subject. That, for instance, conditioned responses of the all-or-none type do not asymptote at unity may be a combination of many effects not presently addressed by statistical learning models: nonoptimum interstimulus interval, nonoptimum motivation, response produced inhibition, incomplete stimulus control, and the very criteria by which a conditioned response is defined.

Most difficult to control, or eliminate, is inhibition. Its effects appear to be a depression of acquisition asymptote, reminiscence following massed acquisition training and spontaneous recovery following extinction training. The author does not foresee at the present time the implementation of a purely analytic approach to the problems posed by inhibition. Rather, it appears more likely that we shall be required to turn to the organism for a specification of inhibition effects, perhaps incorporating empirically derived functions into present models. One alternative is to treat inhibition as being the result of the addition, to s, of a population of response produced stimulus events having the property of not being conditionable to the response.

SUMMARY

Applying Estes's theory of stimulus fluctuation to the sampling behavior of the subject, a model was developed wherein successive samples from the stimulus population are not independent. Instead of drawing successive random samples from the population, the subject selects samples which overlap in content as a function of the degree of trial massing. Implications of the model for the distribution of trials and experimental sessions were discussed.

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(Received August 3, 1959)

Psychological Review
1961, Vol. 68, No. 3, 225–228

HUMAN BEHAVIOR AND ATMOSPHERIC IONS

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Few people in behavioral research are familiar with the nature of atmospheric ions or are aware that they apparently have behavioral effects. However, in a number of experiments by biologists exploring the nature of this new independent variable, behavioral effects have appeared. It is the intention of this paper to draw the attention of psychologists to this new and interesting area by a review of the behavioral observations that have been made in the biological experiments.

Since these experiments were exploratory, many were deficient in instrumentation, in control of interacting variables, and in their lack of a rational framework. Thus, although they do suggest behavioral effects, it is concluded that the magnitude and extent of the behavioral ion effects are not clear.

Nature of Atmospheric Ions

Normally, air contains about 100 to 2,000 ions per cc., with a preponderance of positive ions. However, various factors such as heat sources, operating electronic equipment, radioactive sources, storms, and aerosols can radically change the ion concentration and ratio.

These atmospheric ions, when created, are free electrons and molecular positive ions. Within microseconds they associate with a half dozen polarizable molecules and form what is called a small atmospheric ion. Essentially, they are singly charged electrical condensers with radii of 5×10^{-8} cm. Kingdon (1960) reported that each ion carries chemical and electrostatic energy on the order of 6 electron volts. If these ions become part of a larger molecular mass such as a dust particle, they lose their behavioral and biological effect.

Behavioral Effects

In view of the chaotic and piecemeal nature of the behavioral data, only the patterns of reported effects are summarized, not particular experiments.

Sensation. Bisa (1954a) reported that negative ionization affects the CFF. Lazarev and Bulanova (1937) reported that ionization causes a "fluctuation in central sensitivity and in the coefficient of the restoration of visual purple." Since the bleaching of visual purple and its restoration is critical in vision, the reports suggest that there should be further exploration of visual processes and ionization. In audition, Bisa (1954b) has stated: "Holtzhey proved statistically confirmable connections between changes in atmospheric ionization and tinnitus."

Minehart, David, and Kornblueh (1958), in the course of studying the effect of negative ions on burns, made the clinical observation that the pain in thermal burn cases is apparently reduced. Whether this might be a peripheral or central effect was not considered. But the finding (Silverman & Kornblueh, 1957) that ions affect the EEG is suggestive.

A knowledge of the effect of air ions may also clarify a current conflict in the literature on whether ionizing radiation can be perceived. Some experiments yield results which indicate that the radiation can be perceived whereas others, which have essentially the same experimental conditions, yield results which indicate that the radiation can not be perceived. As was noted, radioactive substances are a source of ions and ions apparently can be perceived (McGurk, 1959; Yaglou, Brandt, & Benjamin, 1933). This, taken with the fact that aerosols can modify ion content of the air, would suggest that possibly ionizing radiation is perceived through the air it ionizes if the aerosol content is low.

Reaction time. Edstrom (1935), in the course of his physiological work, noted that negative ion inhalation decreases

muscle chronaxia. Speicher [1] found that positive ionization increased simple reaction time, whereas negative ionization decreased reaction time. Knoll (1959) on the other hand reported, on the basis of another simple reaction time experiment, that the sign of the ion is not as important as concentration. He found that "different levels of artificial ionization have a clearly different effect upon reaction time." A close to normal concentration tends to reduce reaction time, while larger concentrations tend to increase it. This, of course, is similar to the effect of many stimulants and drugs. The difference between experimental results can probably be attributed to certain uncontrolled variables which have been discussed elsewhere (Frey, 1959). Considering how difficult it is to affect simple reaction time by various agents, i.e., high temperature, noise, the finding of an ion effect on simple reaction time would suggest much more marked effects on choice or discrimination reaction time.

Activity. Herrington and Smith (1935) found that rats in a negative ion environment increased their activity considerably as measured by the activity wheel. Stanley, in an unpublished report, stated that rats in a negative ion environment showed increased activity as compared to control and positively ionized rats.

Vytchikova and Minkh (1959) stated that Lepekhina reported that "negative ions raise the work capacity of athletes as measured by weight lifting and submaximum intensity activity on a stationary bicycle." In their own study, they reported that after 25 days of inhaling negative ions 15 minutes per day, athletes maintained a standard grip on a dynamometer 46% longer than control subjects. After 9 days of ionization, endurance as measured on the treadmill increased 59.5%. However, in the Vytchikova and Minkh experiments there was questionable matching of control and experimental groups.

[1] F. Speicher, personal communication, 1959.

There is also a report (see Footnote 1) that the variability of individual people working for extended periods on the Minnesota Formboard increased when the air was positively ionized.

These reports, taken with that of Nielsen and Harper (1954) that positive ions cause a significant reduction in succinoxidase activity, would suggest significant activity or motor effects.

Learning. Bauer (1953) used three groups of 23 naive male albino rats in a water maze learning experiment. At the beginning of the experiment, their ages ranged from 60 to 170 days. They lived in small chambers, one group in positively ionized air (6,000 ions per cc.), a second group in negatively ionized air (3,500 ions per cc.), and a third group in normal air. Five days after the beginning of ionization and preliminary trials, the subjects were given one trial a day in the maze for 31 days. Statistical analysis of the time and error scores for the last 30 trials indicated no significant differences between groups.

Jordan and Sokoloff (1959) contrasted two age ranges and their effect on learning with and without negative ions. They used a 14-choice T maze with escape from water as motivation. Of the 300 rats, half were 3 months old and the other half averaged 22 months of age. For 3 hours prior to the trials the rats were exposed to a concentration of 8,500 negative ions per cc. They found that the older rats in a normal atmosphere made three times more errors and twice the time score of the young rats. With negative ions both error and time scores for the old rats were reduced by approximately half.

Other explanations for this beside learning include, for instance, that the effect of the ions was on motivation. The report noted:

> The old rats had . . . startle reaction [which] was always delayed. They hesitated to swim, but once started, they swam as fast as the young rats. The delay in the startle reaction was considerably reduced by negative ionization . . . .

It would also be interesting to know what the old rats had been doing for the 22 months prior to the experiment. This is a rather long time to keep rats without using them, and the authors made no mention as to whether the rats were naive. This could be critical in interpreting their results.

Emotion. Good experimental studies on emotional effects are not available. But as an example of what has been found, it has been reported that a woman engaged in working on a formboard was subjected to an alternation of negative and positive ionization. Apparently, as a consequence of the effect of the alternation in ion polarity, which she could perceive, she exhibited a marked emotional disturbance. This effect was not typically found and the fact that she was in her menses may be pertinent. The reason why the occurrence of the menses is possibly significant is the suggestion (Frey, 1959) that stress may be a necessary condition for marked ion effects to appear. Study of the pattern of all reports indicates that stress may be quite important. This view is reinforced by the recently published paper of Krueger and Smith (1960). On the basis of some of their experimental work, they suggest that positive air ion effects are mediated by the release of free serotonin while negative air ion effects depend upon the ability of negative ions to accelerate the enzymatic oxidation of serotonin. Thus, negative ions would have a reserpine type of effect whereas positive ions would have an iproniazid-25 sort of effect. Their evidence is based upon the physiological effects of air ions on the mammalian trachea and is, in brief, that all the trachea effects produced by positive or negative ions can be duplicated by drugs that operate through the release or metabolic blocking of serotonin. Further, they report that these drugs can prevent excesses of positive or negative air ions from exerting their usual effects on the respiratory tract and also, by means of air ions, it is possible to reproduce the effects of reserpine and iproniazid on the respiratory tract without the untoward systemic effects of these drugs.

By this brief review of an unusual area in which psychological work can be done with a new set of independent variables, it is hoped that a systematic body of knowledge on the behavioral effects of air ions, based on well controlled behavioral research, may be stimulated.

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(Received December 30, 1959)

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